Vol. 49, No. 1 January, 1942

Psychological Review

EDITED BY
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PUBLISHED BI-MONTHLY
BY THE
AMERICAN PSYCHOLOGICAL ASSOCIATION, INC.
PRINCE AND LEMON STS., LANCASTER, PA.
AND NORTHWESTERN UNIVERSITY, EVANSTON, ILLINOIS

Entered as second-class matter July 13, 1897, at the post-office at Lancaster, Pa., under Act of Congress of March 3, 1879

PUBLICATIONS OF

THE AMERICAN PSYCHOLOGICAL ASSOCIATION

WILLARD L. VALENTINE, *Business Manager*

PSYCHOLOGICAL REVIEW

HERBERT S. LANGFELD, *Editor*
Princeton University

Contains original contributions only, appears bi-monthly, January, March, May, July, September, and November, the six numbers comprising a volume of about 540 pages.
Subscription: $5.50 (Foreign, $5.75). Single copies, $1.00.

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Review and Bulletin: $11.00 (Foreign, $11.50).
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Bulletin and J. Exp. (2 vols.): $18.50 (Foreign, $19.25).
Review, Bulletin, and J. Exp. (2 vols.): $23.00 (Foreign, $24.00).

Subscriptions, orders, and business communications should be sent to

THE AMERICAN PSYCHOLOGICAL ASSOCIATION, INC.

NORTHWESTERN UNIVERSITY, EVANSTON, ILLINOIS

VOL. 49, NO. 1 JANUARY, 1942

THE PSYCHOLOGICAL REVIEW

TOWARD THE QUANTIFICATION OF PSYCHOLOGY[1]

BY JOHN PAUL NAFE

Washington University

I. INTRODUCTION

A world of earth, air, fire and water is a quantitative world because all substances are conceived to be made up of varying amounts of these 'elements.' Only the four elements themselves are quantitatively unrelated, the differences between them are unmeasureable even in the simplest terms of more and less and hence may be said to be 'qualitative.' The periodic table excludes fire from its new system but contains even a larger number of elements. However it shows one quantitative relationship between the elements themselves, that of their atomic weights. The periodic table is thus an advance in quantification even though atomic weight, at this level, can be only imperfectly understood. It is not apparent that any element can be equated with another by adding to or subtracting from it. Iron and oxygen, for example, at this level, vary quantitatively from each other in atomic weight but iron cannot be so rarefied that it becomes oxygen nor oxygen so compressed that it becomes iron. They are therefore qualitatively different substances. With the modern breaking up of the atom, however, it appears that even iron and oxygen are of the same substance in the sense that both are made from the same sub-elements. The differences between them lie in their numbers of protons, electrons, and neutrons and in the spatial arrangement and velocities of these

[1] Presidential address, read before the Southern Society for Philosophy and Psychology, April 11, 1941.

particles. This, except for details, carries quantification to completion because all substance is reduced to one in the sense that, in principle, any substance can be equated with any other by addition to, or subtraction from, the given. The differences are further regarded as quantitative because the particles can be numbered, their arrangement can be mathematically described by the use of coördinates and their velocities can be quantitatively expressed. Practically the operations by which these measures may be taken are not perfected, or even nearly so, but in theory the variations are all quantitative in the sense that the dimensions in which the variations occur are known and are, theoretically at least, measureable.

Sensory psychology, however, although it quantitatively measures temporal, spatial and intensive differences, is said to remain a qualitative science because sensations show 'qualitative' and 'modal' differences such as those between red and blue, between red and cold, and between red and middle C, differences again which cannot be equated by adding to, or subtracting from, what is given, *e.g.*, one cannot make red resemble blue by adding or subtracting red. To account for such qualitative differences in sensation there must be some selective process, a corresponding nervous mechanism which responds only, or primarily, to the proper or 'adequate' stimulus. Such a selective function usually is assigned to the end-organ rather than to the afferent nerve terminal but without clear definition, and the frequent implication that the afferent terminals are themselves selective is apparent in that the phenomenon of sensory adaptation is explained as a function of nerve. To understand such qualitative relationships it is necessary to clarify two matters: (1) the nature of stimulation; and (2) the bases for differences between sensations. The interest here is not in any new theory but rather in an insistence upon the logical consequences of well established facts. Such facts are most available in the field of common sensation and where stimulation is of the usual kind, involving end-organs, rather than direct stimulation of nerve trunks by injury or electrical shock.

II. Selective Stimulation

An afferent nerve ending, or end-organ, may be highly structured as are the rods and cones of the retina, it may be simpler as in the case of the muscle spindles, it may be a simple fiber ending in relation to an otherwise independent structure such as the hairs of the skin, or the afferent fibers may terminate simply as free-nerve-endings in tissue such as muscle. Always the change in conditions we call stimulation first affects the non-nervous tissues of such terminations and because these differ from each other in structure, composition and location they respond to different influences, and respond in different ways, and such responses, or failure to respond, constitutes 'selection of stimuli.' Thus the fibers in functional relation with the hair-bulbs are excited to action by a slight disturbance of the air. It is not necessary to assume that these fibers differ in any way from fibers terminating between the cells of the skin, but they are adequately stimulated because of the structure and location of the hairs, and other fibers are not. Again the semi-circular canals, because of their structure and location, respond to movements of the head in relation to gravity. Their responses, in turn, activate their afferent nerve terminals. It is quite unnecessary to assume any special property peculiar to these nervous elements to account for their responses. Still again, different tissues of the skin are affected in different ways by a rise in temperature. The blood vessels dilate and the afferent terminals within the vessel walls become active while some others do not, not because of any special property of the nerve, but because of the special properties of the blood vessels. The nerve fibers participate in the selective process only to the extent that their terminals differ in shape, some branching and terminating simply, others showing expansions of many kinds. Presumably such differences in the form of the terminal will have an effect similar to differences in the non-nervous parts of the endings, in that they will be differently affected by movements set up within these tissues. Otherwise there is no direct evidence that afferent terminals differ in ways which can account for selectivity, and no such evi-

dence is needed because all we know of selectivity is fully accounted for by the differences already described.

In the several examples cited, the non-nervous parts of the end-organ stimulate the afferent terminals by moving them. For common sensation this appears always to be the case and it is not limited to common sensation because movement also is the effective stimulus for the semi-circular canals, for the organs of the vestibule, and for the cochlea.[2] Movement is the only stimulus *known* to be 'adequate' to the excitation of afferent nerves through their peripheral terminals or end-organs. If all respond to movement the assumption of different kinds, or different functions, of afferent nerve terminals for common sensation is unnecessary. Selectivity of adequate stimulation by non-nervous tissues violates no established principles because non-nervous tissues themselves differ.

Sensory adaptation has often been accounted for as due to fatigue—the failure of nerve from overwork—but we know by other means that fatigue of nerve in this sense does not occur. This leaves only the non-nervous parts of the end-organ to account for adpatation. For example, a rise of temperature is accompanied by vascular dilatation, but such dilatation does not continue indefinitely. A given rise in temperature will be accompanied by a limited response of the blood vessels and if the temperature is again increased the vessels will again respond. This is a condition of equilibrium between the forces exerted and the resistances encountered, and not a case of exhaustion or fatigue. As long as the vessels are responding the movement excites the af-

[2] In the light of the facts of adaptation and threshold, movement apparently is the only type of stimulation which can account for the senses of taste and smell and for the activation of the organ of Jacobson, in which the nerve elements are wholly inclosed in tissue. The chemical change apparently evokes a movement of the innervated tissues which in turn excites the nervous response, and the movement evoked is capable of variations corresponding to different odors, tastes, etc. The processes which directly excite the rods and cones to activity are not known but it is known that the effects of light upon retinal excitation must be mediated by some intervening process. Although a presumption may have arisen in favor of movement as the mediator in vision, it is not safe to generalize, for the rods and cones are the most highly elaborated sense organs of the body. Movement is a gross term but the changes it involves at and in the nerve are not agreed upon. There probably is a single process adequate to nerve excitation, a process which is set up alike by movement, injury, electric shock, and chemical and perhaps other changes.

ferent terminals of the vessel walls to activity and when the response of the vessel slows, or ceases, the nervous response will still reflect its degree of activity. Adaptation is then a condition of equilibrium between the stimulus and the non-nervous bodily tissues or processes, not directly involving nerve at all. It constitutes a failure of stimulation and not a failure of the nerve (4).

We have then a clear account of stimulation and the functions of selectivity and adaptation. A change in physical conditions—the stimulus—may effect a bodily change depending upon the nature of the stimulus itself and the structure, composition, and location of the particular bodily tissues or end-organ. If such a change is effected, consequent movement may or may not be effectively transmitted to the afferent terminals depending upon the nature of the movement and the nature of the terminal, but nothing other than movement is known to be adequate to the excitation of the fiber endings. Afferent nerves then, and their nerve impulses, are qualitatively alike and a quantitative description of the differences in volleys of nerve discharge constitutes a complete description of neural differences.

Differences Between Sensations

Boring (1), in a qualitative analysis of organic sensation, showed that hunger, thirst, nausea, etc. are not different qualities of an 'organic sense' peculiar to the visceral organs, but are combinations or patterns of pain and pressure sensations. Hunger and nausea, for example, are very different from each other, but different because the patterns of pain and pressure sensations differ, and not because they constitute qualities of sensation other than pressure and pain. Boring has been sharply criticized for his conclusions but there are physiological facts which now can be brought to bear upon them and which furnish other criteria by which we may be aided in judging the correctness or incorrectness of his analysis. The first fact is that the plain musculature of the stomach contains the terminals of afferent nerves usually called 'free-nerve-endings,' and no others except those connected

with the walls of the blood vessels. The second set of facts has to do with the conditions under which hunger and nausea are felt. Hunger occurs only with certain waves of contraction in the muscular walls of the stomach. When the muscle contractions cease, hunger is abolished and will not be felt again until the waves of contraction recur. In the case of nausea there is a spasm at the pyloric end of the stomach; peristalsis is jerky, and likely to be reversed in direction. These activities involve many of the same muscles, and consequently the same nerves, that are activated in the case of hunger. The immediate stimulus to nerve activity, in both cases, is movement but the pattern of muscular activity associated with nausea is quite different from that associated with hunger. Each series of movements will arouse a nervous excitation and the aroused nervous excitations will vary from each other in ways that reflect the differences in the two series of movements. Therefore we are justified in the conclusion that there are neither 'nerves of hunger' nor 'nerves of nausea' but that the activity of the same nerves, activated in both cases by movement, may give rise to sensations either of nausea or of hunger depending upon the nature of the stimulating movement and the consequent pattern of nervous discharges, and Boring's analysis is, to this extent, verified. Similar factual findings run throughout his study of organic sensations. All are patterns of pressure sensations or of pain, or both. On the side of correlation the only stimulus known to be adequate to the nervous discharges correlated with organic sensations is movement, and always the movements of these tissues are characteristic of the tissue in which they occur and of the conditions under which they are aroused. The correlated volleys of nerve discharge reflect these differences in the spatial and temporal relationships between a number of discrete nerve impulses. It is not the point of view of physiological psychology that sensation *is* a nerve discharge but it is assumed, or accepted, that sensation is a function of the nerve discharges correlated with it. It follows that differences between sensations are represented by corresponding differences between nerve discharges and a descrip-

tion of the neural changes is a description, perhaps the best description, of the corresponding changes in sensation.

Sensitivity to warmth and to cold appears to constitute a special case of the sensitivity of plain muscle systems. Of one thing we may be sure: because warmth and cold may be aroused by mechanical stimulation, or reflexly, without appropriate changes in temperature, they cannot be immediately related to the change of temperature of the afferent nerve terminals but the temperature changes must be made effective through the activity of a mediator which can and does fail in the presence of a 'constant' stimulus. It is perhaps fair to say that a preponderance of the evidence, as it now exists, tends to show that sensations of cold are correlated with constriction, and warmth with dilatation, of certain blood vessels (**5**). There is only one kind of afferent terminal in the vessel walls and these facts force explanations of warmth and cold sensations similar to those for hunger and nausea, namely that differences in the movements of the vessel walls in constriction and dilatation set up volleys of afferent nerve discharges which differ in their patterns as the movements differ. On the introspective side, sensations of warmth and cold have not been analyzed into pressure and pain patterns but Goldscheider noted a pressure component in all warmth and cold sensations, and it is well known that extremes of temperature, both warm and cold, may arouse pain without injury to the nerves or other tissues. The quality of 'heat,' correlated with middle intensities of temperature changes, has been introspectively analyzed into pressure and pain (prick) components. The nerves which conduct impulses associated with thermal sensitivity are, then, like other afferent nerves, activated by movement, but the movements adequate to sensations of warmth and cold are restricted to those associated with dilatation and constriction of the blood vessels. The patterns of movement arouse corresponding patterns of nerve discharge and the correlated sensations vary as they do. 'Quality,' for both warmth and cold, refers to the pattern of sensation.

Sensations of this modality occurring in other parts of the body confirm the general principles of stimulation, selective response, patterns of nerve excitation and correlated sensation. Striate or voluntary muscle is equipped with two types of endings of afferent fibers: (1) the spindles of the muscles and tendons which are discharged by active movement of the muscle in contraction and relaxation, and are associated with kinæsthetic sensations; and (2) the free nerve endings, including several modifications of terminals, which are activated by passive pressures, usually from the skin, and are correlated with sensations often referred to as 'deep pressure.' Sensations from both sources, however, are only of pressure and pain types and are aroused only by movement, so there is no fundamental difference between plain and striate muscle either in stimulus conditions or in the basic quality of aroused sensations. We associate a much greater freedom of movement with voluntary than with involuntary muscle. This variability does not exist within the muscle elements (in both cases they only contract and relax), but in the systems of movements which are built upon the simple contractions. The highly structured kinæsthetic sensitivity associated with free bodily movement depends only upon more and more complicated patterns of nerve activity associated with the highly involved systems of movement.

In general the skin is passive. Objects from outside the body impress themselves against it or disturbances within the body, or even within the skin itself, are set up which are adequate to sensation. In any case there must be movement within the tissue to arouse sensation. Some intrinsic movements of the skin have been related to sensations of warmth and cold. Otherwise its sensations usually are characterized by the objects with which it comes into contact. Pressure and pain may be called the standard sensations and their modifications are called smooth, rough, sticky, hard, soft, etc. for the physical characteristics of the usual stimulus. There is no difference of opinion about these experiences; they are all analyzable into patterns of pressure and are correlated with characteristic modifications of movements which reflect the

peculiarities in texture of the stimulus object. Some objects feel 'wet,' some injuries 'smart,' and in examples of this kind we have more complex combinations: in the one case of pressure and cold, and in the other of pain and warmth sensations. Vibratory sensitivity, which is not limited to the skin, is of pressure quality and has been shown by Geldard (2) and others to depend upon movement of the same nervous mechanisms as those involved in pressure sensations. Even tactually perceived movement is aroused by pressure sensations, spaced and timed within limits very similar to those essential to the visual perception of movement. The conditions under which we experience cutaneous tickling and itching resemble those for the tactual perception of movement and these experiences are introspectively similar to tactual movement.

Sensations of 'light touch' or 'contact' have, by some investigators, been differentiated from sensations of pressure because the nerve impulses associated with them show a distinctive speed of conduction, in fact the fastest of all afferent impulses, and are therefore associated with the largest afferent fibers. Now the larger a nerve fiber the more irritable it is and a contact is, by definition, a lighter stimulus than a pressure. Therefore a slight disturbance, inadequate to smaller fibers, will excite the larger fibers to activity, and the lighter the touch the larger the average of the fibers activated. The differentiation of touch from pressure sensations is thus based upon the pattern of discharge, its principle characteristic being determined by the size of the activated fibers. Otherwise the fibers are not known to differ from others. The nature of the adequate stimulus and the basic quality of the resulting sensations are the same as in pressure sensations. Introspectively, experiences of contact are often described as 'light' or 'bright' pressure, and 'bright' pains are often distinguished from 'dull' pains, but these differences in quality are generally believed to be related to pattern differences in the nerve discharges and are of no other importance for the present problem.

Since analysis has been made in terms of pain and pressure sensations, a clarification of the relationships between them,

important for the points at issue, becomes urgent. Because pain so noticeably results from intensive stimulation, it often has been proposed that pain is intense pressure sensation, but the suggestion has been as often rejected upon the introspective evidence that, even allowing for intensive differences, the two do not feel alike. Neither appears to be introspectively identifiable in terms of the other. At the present time they are generally held to be two 'qualities' of sensation and investigators are now trying to isolate pain and pressure end-organs and fibers. There are a few outstanding facts which bear directly upon the existence of separate nervous mechanisms for pain and pressure sensations: (1) Goldscheider, and others, have shown that every part of the body which is sensitive to pain or to pressure sensation is sensitive to both. Many of the areas have only free nerve endings and although functional difference in such similar fibers is possible, it is not known. (2) Pain also can be aroused by severe muscular contraction of the blood vessels when stimulated by extremes of heat and cold. Thus, both the fibers associated with temperature sensitivity and the free nerve endings associated with pressure sensations may mediate impulses associated with pain. (3) The attempt to identify pain fibers by size has failed. Fibers involved in pain were first identified (not differentiated), among the smallest afferent fibers—those of the 'c' elevation of the Erlanger type of analysis—but since that time they have been found among afferent fibers of all sizes and therefore the evidence tends to show that all afferent fibers of this department of sense may carry impulses correlated with both pain and pressure sensations. (4) The evidence from the dissociation of pain and pressure sensations fails to establish the existence of separate fibers because the facts are better explained as reductions in the number of fibers involved, in the intensity of nerve impulses, and in the frequencies of discharge, whether the dissociation is due to disease, trauma or drug. Pain also may be abolished by a hemisection of the afferent nerve from an affected area without any selection of pain fibers. (5) Heinbecker, Bishop, and O'Leary (3), in a series of experiments in which human nerve

was directly stimulated with electric shock, show that a single shock, regardless of its intensity, within the limits of their apparatus, does not produce pain but does produce a painless sensation. With electrical stimulation each shock means one or two impulses aroused in each activated fiber. If the impulses (shocks) are continued above a minimum length of time, and above a minimum rate, pain is aroused. (With the nerve used, not less than twelve shocks at a rate of about 2½ per second were necessary for pain.) These results, translated into terms of normal stimulation, mean exactly that pain is a function of the intensity of stimulation, for with normal instead of electrical stimulation, both the frequency and duration of nerve discharge are functions of the strength of stimulation.

The evidence thus clearly indicates that sensations of pressure and pain are both aroused by the same stimuli and mediated in the peripheral nervous system by the same fibers. Pain requires only a more intense stimulus than does pressure sensation. It does not necessarily follow that pain is *merely* intense pressure sensation because: first, the latency of pain sometimes appears to be too long to be accounted for as the direct effect of stimulation; and second, pain is associated with many reflex effects, such as rise in blood pressure, some of which may be necessary to the arousal of pain, and probably are since the facts concerning the irradiation of pain seem to support such a view. The important facts for us are that at the point of origin pain and pressure sensations are aroused by the same kind of stimulation and that throughout the peripheral nervous system pain and pressure sensations are associated with activity of the same nerve fibers. In the nerves of origin the differences between nerve discharges correlated with pain and pressure sensations lie in the patterns of nerve discharge, *i.e.*, in the number of nerve impulses aroused per unit of time (frequency of discharge), and the number aroused per unit area (density of discharge).

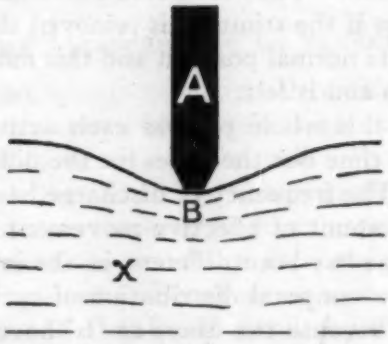
To distinguish pain from pressure sensation by differences in the patterns of correlated nerve discharge may be confusing because 'pattern' is here used in a sense slightly different

from that used to distinguish more complex sensations. An illustration may clarify these relationships: Drawings made with the same pencil of two different subjects may be compared with two different sensations such as hunger and warmth. The differences between the two pictures we call differences in pattern because they depend only upon the spatial distribution of lines. (Sensations, of course, involve both temporal and spatial distributions.) If we now examine the lines of which the two pictures are composed, some appear black and others gray, and these differences we may compare to those between pain and pressure sensations. To say that the lines themselves vary only in pattern is to use the word 'pattern' upon a somewhat different level than before, but it is correct in that the difference between dark and lighter lines also is only a matter of spatial distribution, *i.e.*, the density, of graphite particles. Pain and pressure sensations differ similarly in the frequency and density of nerve impulses. In such situations we are not conscious of either the individual particles of graphite or of nerve impulses, as such, but by special means we may observe that both vary in size and, in both cases, this again is merely a matter of distribution, of spatial concentration, *i.e.*, differences only in pattern.

To say that pain and pressure vary only in pattern does not convey a concrete idea of their differences. If the nervous excitation associated with a mild pressure may be represented as a little water from a garden sprinkling can, pain might be represented by the stream from a fire hose. Both may be regarded as patterns of drops of water which vary in temporal and spatial relationships. Pain is *primarily* intensive pressure sensation. It may or may not involve reflexly aroused modifications, but if it does their sensory consequences are not basically different from those aroused by other movements. Reflexly excited modifications of pattern such as those resulting in, or modifying, pain, although more noticeable, do not affect our problem any more than the reflexly excited affective and emotional states which occur with sensation. The best evidence indicates that affective and

emotional states also are patterns of pressure sensations induced by reflexly initiated patterns of bodily movements.

We come to a consideration of the basic quality of pressure. A simple illustration is used but any other mode of arousal of pressure sensations would do as well. The figure represents a stimulus 'a' in contact with an area of the skin 'b,' and 'x' is any one of a myriad of afferent nerve terminals within the skin. This stimulus sinks by its own weight and as it sinks the nerve endings nearest the area 'b' will be moved at a maximum rate. Afferent terminals farther from



area 'b' also will be moved, and excited to activity, but the farther from the area 'b,' the slower the movement and the fewer responding fibers there will be. That is an oversimplified cross section of events for a given moment but the operation continues. In the first moment the stimulus will sink at its maximum rate. As it continues and the total volume of displaced tissue increases, the downward rate of movement of the stimulus becomes slower, and hence movement throughout the excited area decreases. The movement of the fiber ending at 'x' in relation to the tissue in which it is embedded is critical for sensation and as resistance increases the excursion of tissues is less but movement of 'x' in relation to the tissues immediately surrounding it, and its own tissue, becomes greater and sensation tends toward pain. 'x' is moved more slowly at first and increases in speed as the stimulus approaches it, but its speed is subject to the slowing of the stimulus itself. As the movement as a whole slows,

more and more of the active fibers cease to discharge and as it falls below a minimum rate for the activation of nerve, even the fibers now nearest the area 'b' cease to discharge and we say that adaptation is complete. A state of equilibrium between the force of the 'constant' stimulus and resistance of the tissues has been reached and consequently there is no further adequate movement. Stimulation has failed and sensation has ceased. Neither the mere presence of a constant stimulus, nor of a pressure gradient, is effective for sensation and so a 'constant' stimulus does not give continuous stimulation. That the nerve fibers have not failed is apparent because if the stimulus is removed the elastic tissue returns toward its normal position and this movement excites a nerve response and is felt.

Throughout this whole process each activated fiber has discharged for a time but the times for the different fibers are not the same. The frequency of discharge has varied in each fiber with the amount of effective movement at its terminal and the discharge has been different in the individual fibers. There has been a temporal distribution of excited fibers with its focus at 'b,' because the fibers at 'b' have discharged all the time and others shorter times depending upon their distance from 'b,' and there has been a spatial distribution with its focus also at 'b' because the tissues at 'b' have been moved the most. All of these temporal and spatial relationships at the periphery are reflected in the discharge as a whole in the nerve which contains the separate fibers, and this constitutes a 'pattern of discharge.' Such patterns will vary with different conditions of stimulation, *i.e.*, with different weights of stimulus, with areas of different size, and with different tissues. So even the slightest pressure sensation is correlated with both a *pattern* of stimulating movement and a consequent *pattern* of nerve discharges. It is itself a *pattern* of sensation and it varies with the conditions under which it is aroused. The word 'quality' refers only to such a moment by moment representation of events at the periphery. Such experiences are relatively simple and hence convenient to use for purposes of comparison, but to state that all sensa-

tions of this modality are of one quality is only to say that they are all basically alike. The one basic similarity, however, is the characteristic of the modality and not of any one pattern or quality of it. What all of these sensations have in common is that they are felt, they are (common) sensed, and the particular quality, the pattern of sensation, is impressed upon this common basis. Sensation, then, is not a message about events at the periphery of the body; it is a picture of them, a representation in nerve impulses, small points like a half-tone, but it is a 'moving picture' in that changes are pictured continuously as long as they continue to occur.

The Measurement of Sensation

In the measurement of temporal, spatial, and intensive dimensions of sensation, observations are taken at intervals from a quantitative series of stimuli. We do not assume that sensation is limited to the observations made or by the ability of an observer correctly to describe each difference, but by statistical methods we secure data and by interpolation we construct a graded and continuous series of sensations to fit the gradations of stimuli and correlated variations in nerve response. This is called 'quantitative psychology' and the aspects of sensation studied are said to vary quantitatively. When, instead of varying simple factors such as weight and area, the *form* of stimulation is changed by inducing a wave motion within the tissue, *e.g.*, a series of muscular contractions, stimulus conditions become relatively complex, but no less quantitative. The differences in the volleys of nerve discharge will reflect the complicated movement of tissue and they may still be expressed in terms of number of fibers, frequencies of discharge, etc., all quantitative concepts and only more highly patterned, more complex, than those associated with simpler sensations. The consequent differences in sensation are described by an observer less accurately and less adequately, but the same may be said of the mathematical description of irregular geometrical figures. Such difficulties do not make the differences any less quantitative. All bodily movements and all volleys of nerve discharges can be quan-

titatively described. Hence the differences between movements set up by contraction of muscle, such as hunger waves, and those set up by the force of an object in contact with the body, such as simple pressure, vary quantitatively, the consequent patterns of nerve responses vary quantitatively, and the correlated sensations vary as the patterns of nerve responses vary.

Modality Differences

Aristotle's criterion of modality was the ability to discriminate them, an ability in which, he said, "the senses are not deceived," but it appears that the senses are deceived between smell and taste. Subsequent attempts to define modalities by their sense-organs and fiber tracts, and by their 'adequate stimuli,' also failed. Yet there is another possible criterion. We know that when the afferent nerves of common sensitivity, unattached at the peripheral end, are directly stimulated a sensation is felt; it has been reported that in the case of removal of both eyes, direct stimulation of an optic nerve gives a sensation of light and this evidence, if accurately reported, indicates a central basis for modality differences. At present we must assume that the fibers associated with smell, taste, and hearing, if directly stimulated, also would each give an unformed, sensory experience of its own characteristic modality. Although direct factual support is meagre, this view is widely held and is consistent with many observations. Without questioning the view or presenting other possibilities, it is of some interest to see how it fits our facts.

Under this view the sensory areas of the brain are sometimes regarded as nervous mechanisms ready to be activated but there also is much evidence of intrinsic activity of the nervous system. In the periphery we are familiar with 'resting frequencies' of nervous discharge and within the central nervous system there are detectable rhythms of change of electrical potentials which indicate continuous nervous activities. Psychological evidence of such continuous activity is almost limited to 'central gray' in vision but there are scat-

tered observations upon differences between temporary lack of activity in a sense department and total absence of function. We know that in some cases the activities shown by different central areas are not the same and each of the areas associated with a sense modality may have a characteristic, intrinsic, nervous activity. So, instead of initiating an activity within the central nervous system, incoming volleys of nerve impulses may modify an already existing activity. In either case sensation, in all of its aspects, is aroused within the central nervous system when the nervous activity within the area of a given modality is molded, or modified, by the arrival of an excitation which began in the peripheral nervous system as a volley of nerve impulses, patterned by the conditions under which it was aroused. The pattern, or quality, of the sensation, *i.e.*, its spatial and temporal aspects, is determined by the pattern of the volley of nerve impulses, but the modality furnishes, or directs, the activity upon which the peripheral pattern is impressed. In either case, modality differences must be determined within the nervous system by differences in nervous activity and such differences, as far as the facts now take us, may always be quantitatively expressed.

The quality of sensation, then, is a pattern of sensation correlated with an activity within the central nervous system which results from, or is modified by, a correlated volley of nerve impulses from the periphery. Variations may occur in both the temporal and spatial aspects of the volley, and when such variations are sufficiently gross they are sensed as temporal and spatial changes in sensation and described in quantitative terms. When too minute, or too involved, for such realization they are still sensed as changes in the pattern of the sensory experience and the details of such patterns may be quantitatively described in terms of the nervous discharge. One may object, after all this is said, that red still is different from blue, or cold, or pain, in ways that do not appear to be quantitative, but that is beside the point. Our world of earth, air, fire and water, also, still appears to be dominated by the undescribable differences between these elements, if we must think upon this level. For all our quantification in

physics, oxygen still cannot be compressed to make iron, or iron rarefied to make oxygen, but this problem is misstated because it is irrelevant to the newer concept. Quantification is not made possible by observations alone, for we know nothing of atoms or nerve impulses by such means. Quantification is realized only through logical processes after consideration of all the pertinent, experimentally observed, facts.

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[MS. received May 1, 1941]

THE CREATIVE YEARS: OIL PAINTINGS, ETCHINGS, AND ARCHITECTURAL WORKS

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I. Introduction

What are man's most creative years? What are the chronological ages at which men are most likely to design great architectural works and to execute their most notable oil paintings and etchings? In the writings of those who have attempted to answer these questions four principal fallacies, or weaknesses, are to be found. The fallacies may be classified as follows: (1) Mere assertions, or assumptions, unsupported by quantitative evidence, that men do their best work at this or at that particular age level, (2) quotations from earlier writers whose names may have great prestige value but whose quoted statements are likewise unsupported by quantitative evidence, (3) conclusions which are bolstered by the citation of one or a very few exceptional cases, and (4) claims which are sustained by a procedure which the propaganda analysts have called 'card-stacking.' When the latter procedure is followed all data are ignored except those which produce the best possible case for a given writer's preconceived prejudice.

If the validity of a given thesis could be established merely by making unsupported assertions, by quoting writers who have great prestige, or by the selection of a few exceptional cases and the omission of all others, it would be possible for anyone to 'prove' the validity of almost any thesis whatsoever. In previous articles (7, 9, 10, 11, 12, 13) the fact has been emphasized that, for rigorous study of man's creative years, a method more adequate than any of the foregoing is required—a method which takes account of both exceptional and of typical instances. Obviously, the relationship between any kind of performance and the chronological age level at

which that performance is most likely to occur can be ascertained only if enough random data are assembled to yield a frequency distribution. In the present study the writer has sought to do this for oil paintings, for etchings, and for architectural works.

When age differences in artistic creativity are being studied, proper allowance should be made for a number of variables. For example, some artists have died early; others have died in middle age. Since the representatives of the younger age groups are always likely to be more numerous than are the representatives of the contemporary older age groups, the younger age groups might conceivably accomplish more creative work merely because of their greater numerical strength. In the present study this difficulty has been overcome by computing the *average* number of great works that were executed by individuals within each of the age groups.

Several additional facts should be kept in mind. It is not possible to study the entire life work of persons who are still living and achieving. There is no sure way of knowing what the living individuals may accomplish during their later years. The present study includes for the most part, therefore, data for deceased persons only.[1] For these deceased individuals the record is complete, and future research will probably change it only slightly, if at all.

In making the present study it was found that in some few instances the authorities disagreed as to the year during which a particular painting was executed. In some other cases there was disagreement regarding the birth or the death date of the artist. In all such instances it seemed best to omit both sets of dates.

II. Age Differences in Artistic Creativity

Famous oil paintings.—Data for Fig. 1 were obtained from *The World Book of Organized Knowledge in Story and Picture* (16). This latter publication attempts to list 100 of the world's most famous oil paintings. It seems safe to assume

[1] There will be some exceptions to this rule. These exceptions will be noted when they occur.

that this list of famous paintings was obtained without reference to any such incidental factor as the ages of the artists at the time they executed the 100 paintings. For the study of age differences in creativity, a list of famous paintings thus selected is all the more useful, since such a list is likely to reflect no bias, intentional or unintentional, in so far as the age factor is concerned.

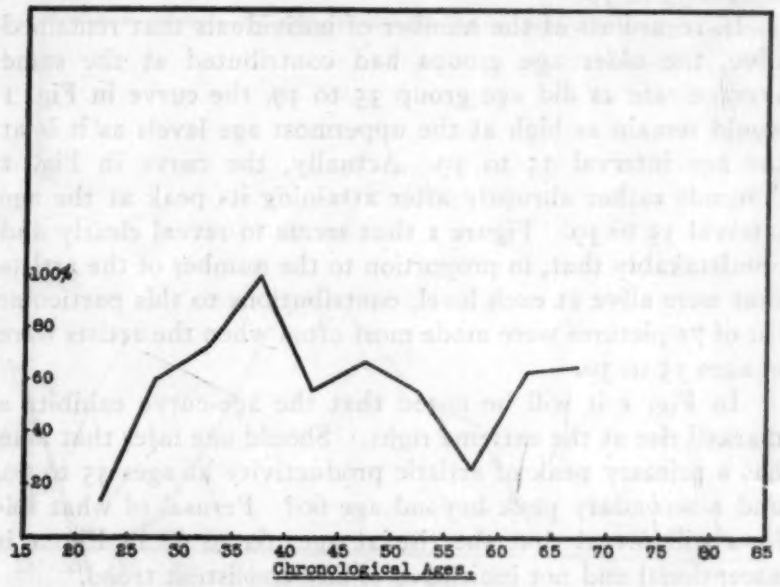

FIG. 1. One hundred of the world's famous paintings. 72 paintings by 42 deceased artists. Data from *The World Book of Organized Knowledge in Story and Picture* (16).

Of the 100 oil paintings that were listed in *The World Book of Organized Knowledge* the present writer was able to find dates of execution for 72 of the works by 42 deceased artists. For these 72 paintings, Fig. 1 sets forth the average number that was executed during each five-year interval of the artists' lives. Since Fig. 1 sets forth the *average* number of paintings that was executed during each five-year interval of the artists' lives, this age-curve makes proper allowance for the fact that more of the painters were alive at the younger than at the older age levels. For example, in obtaining the data that are

set forth in Fig. 1, it was found that, at the age interval 35 to 39 inclusive, the average number of pictures per artist was 0.0700, whereas, at the age interval 40 to 44 inclusive, the average had fallen to 0.0389. In Fig. 1 the age-curve is so drawn as to be only 0.0389/0.0700 as high at ages 40 to 44 as at ages 35 to 39, thereby indicating that average productivity was only about three-fifths as great at ages 40 to 44 as at ages 35 to 39.[2]

If, regardless of the number of individuals that remained alive, the older age groups had contributed at the same average rate as did age group 35 to 39, the curve in Fig. 1 would remain as high at the uppermost age levels as it is at the age interval 35 to 39. Actually, the curve in Fig. 1 descends rather abruptly after attaining its peak at the age interval 35 to 39. Figure 1 thus seems to reveal clearly and unmistakably that, in proportion to the number of the artists that were alive at each level, contributions to this particular list of 72 pictures were made most often when the artists were of ages 35 to 39.

In Fig. 1 it will be noted that the age-curve exhibits a marked rise at the extreme right. Should one infer that man has a primary peak of artistic productivity at ages 35 to 39, and a secondary peak beyond age 60? Perusal of what follows will reveal that the rise at ages 62 to 67 in Fig. 1 is exceptional and not indicative of any consistent trend.

Architectural works.—An attempt was made to ascertain the chronological ages at which famous architects constructed the chief buildings of the Italian Renaissance. It was impossible for the present writer to obtain dependable data for

[2] In constructing the graphs that accompany this article, the data for each of them were first reduced to a comparable basis by the following procedure: The peak of each statistical distribution was arbitrarily assigned a value of 100 per cent and the other averages within the same statistical distribution were assigned proportionate percentage values. For example, in Fig. 1, the peak of the distribution occurred at ages 35 to 39 inclusive. At this latter age interval the average number of paintings per artist was 0.0700. In Fig. 1 the decimal 0.0700 is plotted therefore as 100 per cent. At the age interval 40 to 44 inclusive, the average number of paintings per artist was 0.0389. This latter decimal fraction is equivalent to 56 per cent of the maximum production (56 per cent of 0.0700) and in Fig. 1 the decimal 0.0389 is plotted therefore as 56 per cent. The foregoing method of plotting should be borne in mind when studying the graphs.

the works of these or of any other early group of architects. It was therefore necessary to confine this part of the present study to more recent architectural works. Data for the construction of Fig. 2 were obtained from *Die Baukunst der Neuesten Zeit* (14). Figure 2 presents information regarding 293 examples of recent notable architectural works by 134 individuals, 121 of whom are still living. Since only 13 of

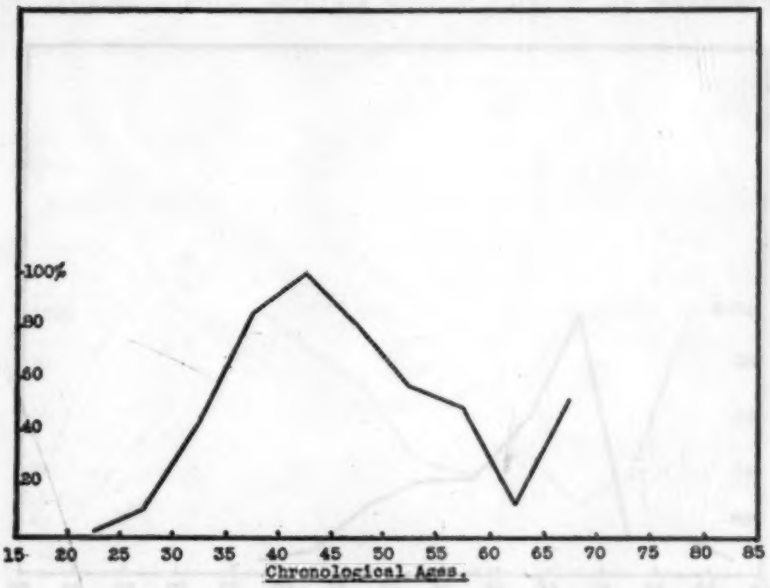

FIG. 2. Outstanding examples of recent architecture. 293 edifices by 134 individuals, most of whom are still living. Data from G. A. Platz's *Die Baukunst der Neuesten Zeit* (14).

the 134 architects had died prior to the publication of Platz's book, there were not enough deceased individuals to justify the construction of a separate age-curve for the works of deceased architects only. Figure 2 suggests, however, that these living architects have designed their notable edifices most often when they were from ages 40 to 44 inclusive.

Etchings.—Figure 3 presents information (6) regarding modern etchings that were to be found in the National Art Library of the Victoria and Albert Museum,[3] of London,

[3] Formerly called the South Kensington Museum.

England, in the year 1903. Figure 3 presents data for 162 dated etchings by 27 deceased artists. In constructing Fig. 3 the etchings of *foreign* schools only were employed on the assumption that the works of the foreign artists were likely to be very select specimens. The peak of the curve in Fig. 3 suggests that very select etchings have been executed most frequently by artists when the latter were from ages 30 to 34 inclusive.

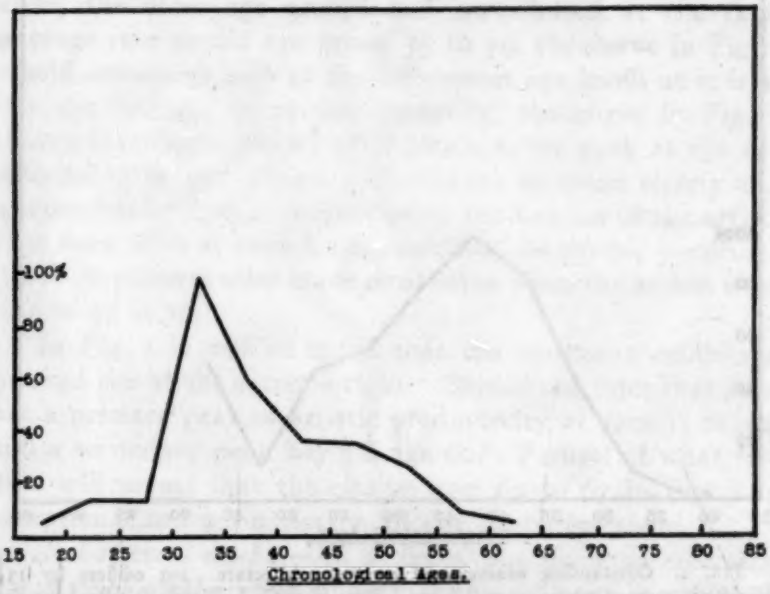

FIG. 3. Modern etchings of the foreign schools in the National Art Library: Victoria and Albert Museum. 162 dated etchings by 27 deceased artists. Data from the *Catalogue of Prints* (6).

The important pictures of the Louvre.—Two books (8, 15), each of which attempts to list the more important pictures of the Louvre, were consulted. It was possible to obtain adequate data for 43 of the pictures which were the work of 32 deceased artists. Figure 4 sets forth the contributions of the various age groups. In constructing Fig. 4 each painting was counted as often as it was listed in the two books that were studied. Thus, if a given painting was listed in only one of the two books, that particular painting was counted only once

in the construction of Fig. 4. But, if the painting was listed in both books, the painting was counted twice in constructing Fig. 4. The foregoing procedure is based upon the assumption that those paintings which were listed by each of the two authors probably possess greater artistic merit than do those which were listed by only one of the authors. This measure of merit, although it is obviously a crude one, is perhaps sufficiently accurate for the purpose for which it is

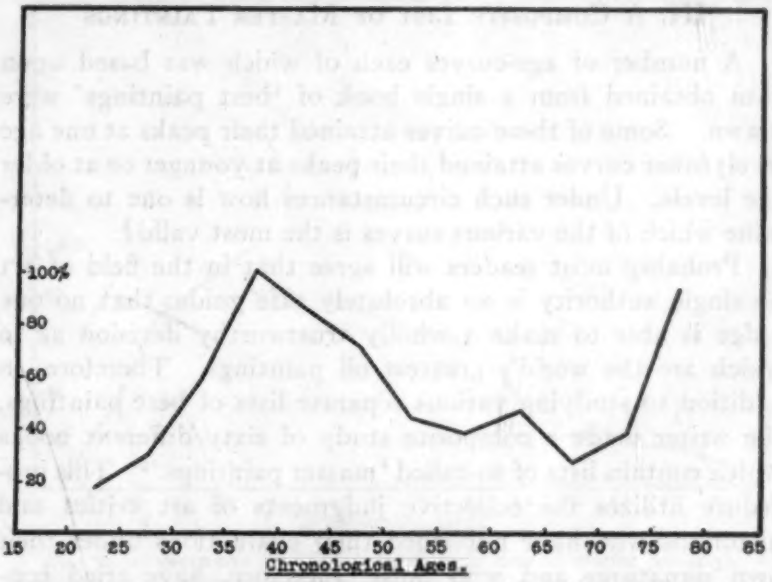

FIG. 4. The important pictures of the Louvre. 43 paintings by 32 deceased artists. Data from F. Heywood, *The important pictures of the Louvre* (8), and from M. K. Potter, *The art of the Louvre* (15).

here used. In any event, there is no valid reason for supposing that the foregoing procedure is prejudicial to any one particular age group.

Figure 4 is similar to Fig. 1 in that it attains its peak at ages 35 to 39. Figure 4 also reveals a secondary peak at the extreme right; but, in this instance, the secondary peak occurs at ages 75 to 79 instead of at ages 62 to 67. Detailed analysis reveals that the rise at the older age levels in both Figs. 1 and 4 is due chiefly to the fact that the number of individuals

alive at these advanced age intervals was extremely small. When an average is computed for the achievements of a very small number of individuals, the average is not dependable. It will be shown in the paragraphs which follow that, when the number of artists at the older age levels is reasonably large, an age-curve which sets forth data for creativity in the field of painting exhibits no marked tendency to rise at any of the uppermost age levels.

III. A Composite List of Master Paintings

A number of age-curves each of which was based upon data obtained from a single book of 'best paintings' were drawn. Some of these curves attained their peaks at one age level; other curves attained their peaks at younger or at older age levels. Under such circumstances how is one to determine which of the various curves is the most valid?

Probably most readers will agree that in the field of art no single authority is an absolutely safe guide: that no one judge is able to make a wholly trustworthy decision as to which are the world's greatest oil paintings. Therefore, in addition to studying various separate lists of best paintings, the writer made a composite study of sixty different books which contain lists of so-called 'master paintings.'[4] This procedure utilizes the collective judgments of art critics and historians who have published their evaluations under their own signatures and who must, therefore, have tried conscientiously to make sound evaluations.

In what follows it is assumed: (1) That this large number of independent critics have exhibited no constant prejudice for or against any one particular age group, and (2) that careful study of the frequency with which paintings have been listed by the various compilers should enable one to identify the really great oil paintings. The composite list of pictures

[4] The sixty lists were compiled by both European and by American critics. The dates of publication for the lists range from 1890 to 1938. When arranged alphabetically the list of sixty books starts with E. R. Abbott, *The great painters in relation to the European tradition*, New York: Harcourt, Brace & Co., 1927, and ends with R. C. Witt, *One hundred masterpieces of painting*, New York: Frederick A. Stokes Co., 1937.

that was obtained by the procedure outlined above included a total of 1,684 paintings by 305 deceased artists. When each of the 1,684 paintings was tallied once only, regardless of how many times it had been mentioned in the sixty lists of master paintings, the resultant age-curve exhibited a very marked rise at ages 90 to 99. When each of the 1,684 paintings was tallied as many times as it was listed in the sixty books of superior paintings, thus giving greater weight to the

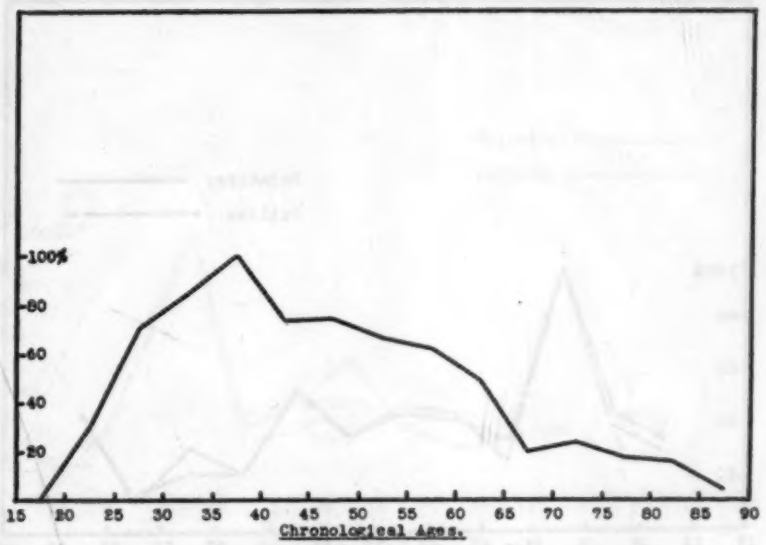

FIG. 5. A composite study of more than 60 lists of master paintings. 650 paintings each of which appeared two or more times in the composite list. In the above graph each painting was counted as many times as it was listed.

paintings which probably possess the greater artistic merit, the resultant age-curve still revealed a marked rise at ages 90 to 99, but this rise was now much less marked than had been the case when each painting was tallied once only, regardless of its probable merit.

Figure 5 was constructed by omitting all pictures which were listed once only in the sixty books, and by crediting or tallying each of the remaining 650 pictures as many times as it was listed in the sixty books of master paintings. In constructing Fig. 5 pictures which were found to be listed only

once in the sixty compilations were omitted on the theory that this procedure would tend to eliminate eccentric individual judgments. That is to say, if a given painting were judged to be a great artistic work by only one of the sixty compilers, it was assumed that that particular painting would be less likely to possess genuine merit than would another painting that had been chosen by two or more of the sixty compilers. In the construction of Fig. 5 it was assumed also

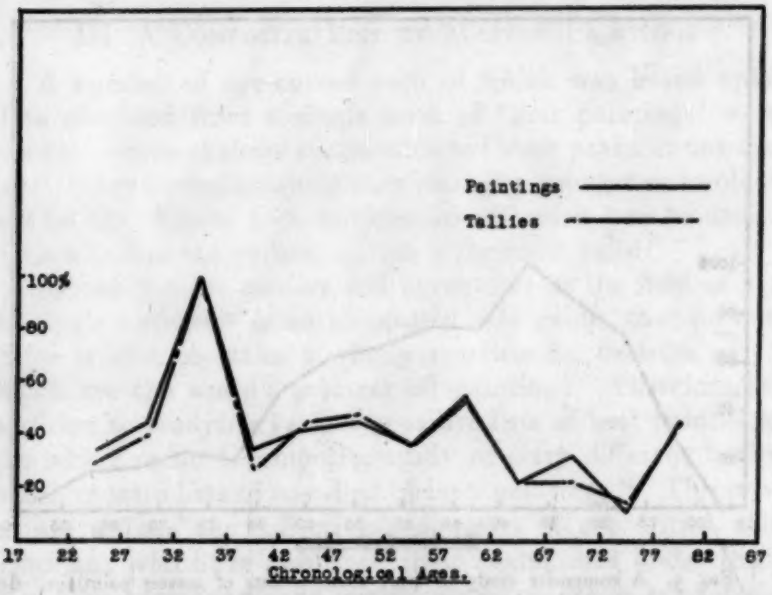

FIG. 6. The one best painting of each of 67 major artists. (*A*) Each painting counted once only. (*B*) Each painting counted as many times as it was listed.

that those paintings which were listed by a large number of the compilers would probably possess greater artistic merit than would other paintings which had been listed by a small number of the compilers.

Figure 5 thus presents data for 650 pictures each of which was listed by two or more of the compilers of the sixty lists of master paintings. The 650 pictures, which collectively were listed a total of 1,891 times, were the work of 168 deceased artists. It will be noted that Fig. 5 exhibits a definite peak at ages 35 to 39, and that in this curve no secondary peak

occurs at the older age levels. It seems evident that when a rise occurs at the uppermost age levels in such curves as are presented herein, the rise is likely to be due either to the small number of cases or to the eccentric judgment of some individual compiler. The consensus of experts suggests, on the other hand, that the best oil paintings have been executed most frequently by artists who were from ages 35 to 39.

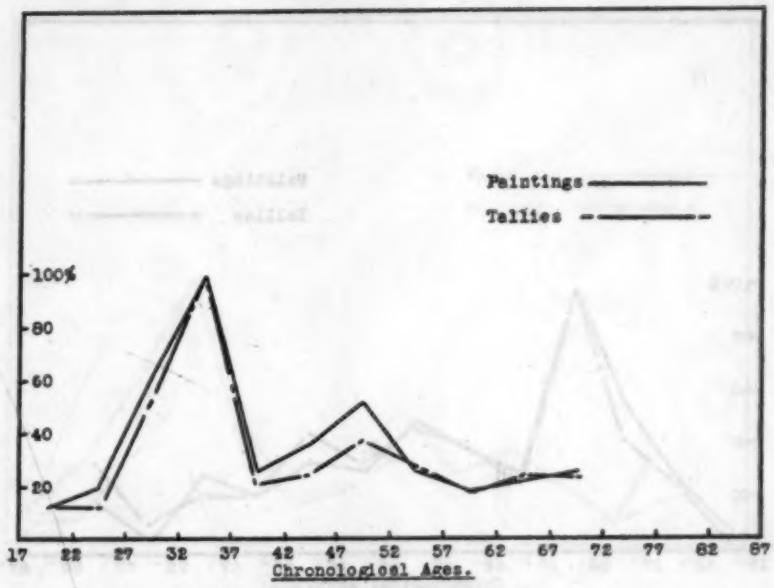

FIG. 7. The one best painting of each of 86 minor artists. (*A*) Each painting counted once only. (*B*) Each painting counted as many times as it was listed.

An artist's one best painting.—For each artist who painted at least one picture which appeared as many as 5 or more times in the composite list of 1,684 paintings, the artist's one best painting was ascertained. In selecting the artist's one best picture, it was assumed that the one painting by a given artist which appeared most frequently in the sixty lists was that particular artist's one best work. In this manner the 67 best paintings of 67 deceased major artists were ascertained. In constructing the solid line of Fig. 6, each of the 67 paintings was tallied only once regardless of how many times it had appeared in the sixty lists. In constructing the

broken line of Fig. 6, on the other hand, each of the 67 paintings was counted as many times as it had appeared in the sixty lists. It will be noted that both of the two curves of Fig. 6 attain their peaks at ages 32 to 36 inclusive, and that these two curves are quite similar in every other respect. The reader may have noticed that in Fig. 6 the age intervals differ from the age intervals that were employed in Figs. 1 to

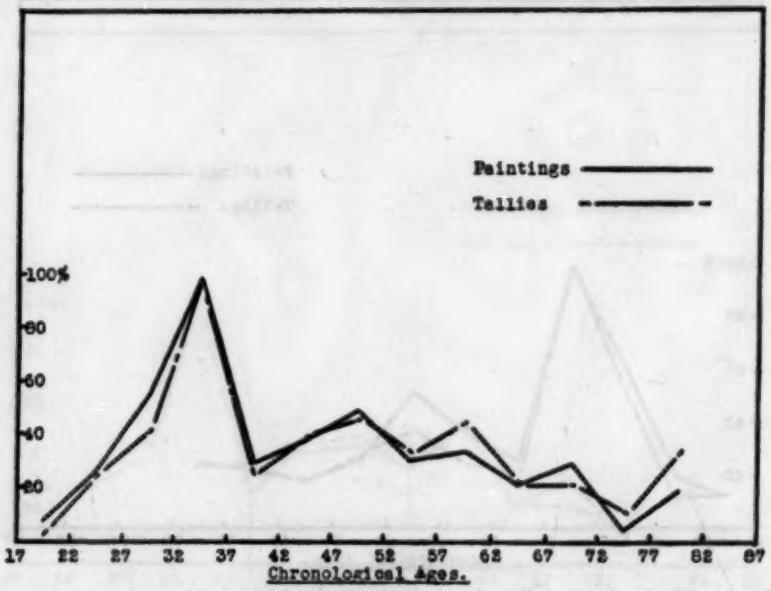

FIG. 8. The one best painting of both major and minor artists. 153 paintings by 153 deceased artists. (*A*) Each painting counted once only. (*B*) Each painting counted as many times as it was listed.

5 inclusive. In Figs. 1 to 5 inclusive, age intervals 20–24, 25–29, and so forth were employed. Detailed study of the data that were used for constructing Fig. 6 revealed rather conclusively, however, that the great oil paintings were executed most frequently at ages 32 to 36 inclusive. In order to set forth this latter fact most clearly, the usual age intervals were abandoned and the intervals 17–21, 22–26, and so forth were employed in the construction not only of Fig. 6 but also of Figs. 7 to 10 inclusive.

For 86 minor artists, *none* of whom had a painting that appeared as often as 5 times in the composite list of 1,684 paintings, the artist's one best picture was identified in the manner that has been described in the preceding paragraph. Figure 7 presents the findings for these 86 minor artists. In Fig. 7, as in Fig. 6, the solid line was drawn by tallying each painting only once, and the broken line in this figure was

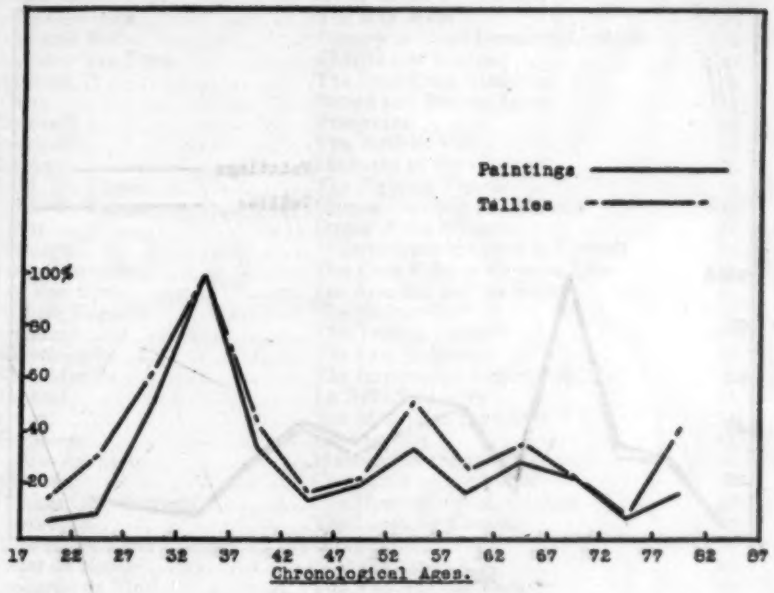

FIG. 9. The one best painting by 61 deceased artists each of whom lived to be 70 or more years of age. (*A*) Each painting counted once only. (*B*) Each painting counted as many times as it was listed.

drawn by counting each picture as many times as it had appeared in the sixty lists. Figure 7 corroborates the findings that are set forth in Fig. 6 in almost every respect. For both the major and the minor artists the one best painting seems to have been executed most frequently when the artists were from ages 32 to 36. This finding is all the more impressive when it is remembered that Figs. 6 and 7 set forth data for two entirely different groups of men. Each of the four curves of Figs. 6 and 7 attains its apogee at ages 32 to 36 inclusive. And in both graphs the peak of the curve is slightly narrower

and the decrement is slightly more abrupt when an attempt is made to give greater weight to the paintings which are probably of superior quality. These two phenomena will be noted also in Fig. 8 in the construction of which the data that were employed in Figs. 6 and 7 have been combined.

It may be supposed by some persons that the very marked decrements at the older age levels in Figs. 6, 7, and 8 are due

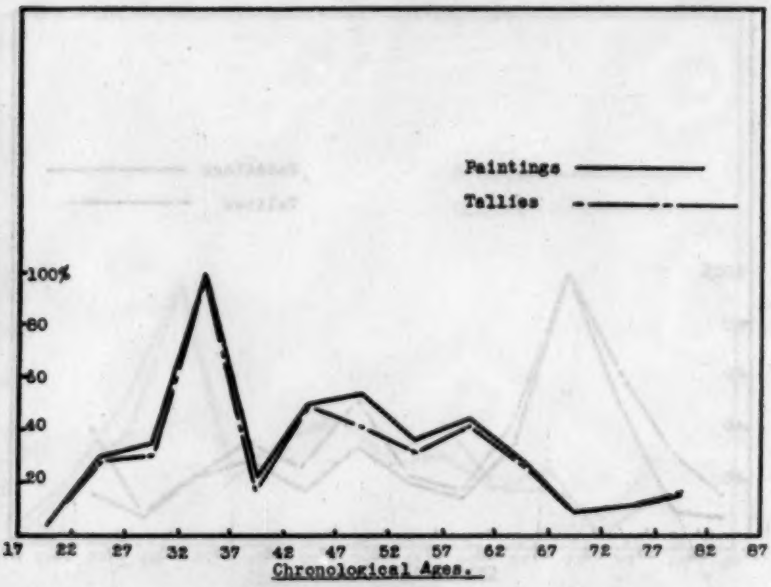

FIG. 10. Age-curves for oil paintings each of which appeared 10 or more times in the composite list. 40 paintings by 27 deceased artists. (*A*) Each painting counted once only. (*B*) Each painting counted as many times as it was listed.

largely to the fact that many of the world's most famous painters have died at relatively early ages. It is true that many great painters have died while they were still young. However, it was (and still is) the belief of the present writer that the computation of *average* productivity at each successive age interval makes adequate allowance for the fact that some artists have died at relatively youthful ages. Data were, nevertheless, obtained for a group of artists all of whom had lived to a ripe old age. For 61 such artists each of whom had lived to be 70 or more years of age the artist's one best

PAINTINGS WHICH APPEARED TEN OR MORE TIMES IN THE COMPOSITE LIST[5]

Name of Artist	Name of Painting	Frequency of Mention
Leonardo da Vinci	Mona Lisa	22
Jean François Millet	The Gleaners	20
Leonardo da Vinci	The Last Supper	18
Jan and Hubert Van Eyck	The Ghent Altar-piece*	17
Thomas Gainsborough	Mrs. Siddons	17
Meindert Hobbema	The Avenue, Middleharnais, Holland	17
Raphael	The Sistine Madonna	17
Corregio	The Marriage of St. Catherine	17
Rembrandt	Sortie of the Civic Guard	16
John Constable	The Hay Wain	15
Giovanni Bellini	Portrait of Doge Leonardo Leordano	14
Anthony Van Dyck	Charles I of England	14
Raphael	The Gran'Duca Madonna	14
Titian	Sacred and Profane Love	14
Botticelli	Primavera	13
Botticelli	The Birth of Venus	13
Raphael	Madonna of the Chair	13
J. M. W. Turner	The Fighting Temaire	13
J. M. W. Turner	Ulysses Deriding Polyphemus	13
Corot	Dance of the Nymphs	12
Watteau	L'Embarquement pour la Cythere	12
John Constable	The Corn Field or Country Lane	11
Jan Van Eyck	Jan Arnolfini and his Wife	11
William Hogarth	The Shrimp Girl*	11
Masaccio	The Tribute Money*	11
Michelangelo	The Last Judgment	11
B. E. Murillo	The Immaculate Conception	11
Raphael	La Belle Jardinière	11
Titian	The Man With the Glove	11
Velasquez	Las Meninas	11
Andrea del Sarto	Madonna of the Harpies	10
Botticelli	Coronation of the Virgin	10
Thomas Gainsborough	The Honorable Mrs. Graham	10
Franz Hals	The Laughing Cavalier	10
Hans Holbein the Younger	George Gieze	10
Pieter de Hooch	A Dutch Interior	10
Leonardo da Vinci	The Virgin of the Rocks*	10
Paul Potter	The Young Bull	10
Rembrandt	Christ at Emmaus	10
Sir Joshua Reynolds	The Age of Innocence	10
Titian	Bacchus and Ariadne	10
Titian	The Tribute Money	10
Velasquez	Surrender of Breda	10
Veronese	Marriage at Cana	10

* Dates of execution unknown.

painting was ascertained in the manner that has been previously described. Figure 9 presents the findings for this group of 61 long-lived artists. It will be noted at once that

[5] This table reveals that only one oil painting appeared in as many as 22 of the sixty books. This apparent lack of unanimity on the part of the critics is due in part to the fact that some of the sixty books dealt only with the art of some one country. Other books dealt only with the paintings of a particular period and some few attempted to list only the more important pictures of a single art gallery such as the Louvre.

Fig. 9 does not differ greatly from Figs. 6, 7, and 8. Figure 9 reveals that even the long-lived artists have executed their one best painting most frequently at ages 32 to 36. Figure 9 thus forces the conclusion that in Figs. 6, 7, and 8 the very marked decrements at the older age levels are not due to the fact that some of the world's foremost painters have died at or before their prime.

The world's forty 'best' oil paintings.—Figure 10 presents data for 40 oil paintings each of which appeared 10 or more times in the composite list of 1,684 oil paintings. The 40 paintings were the work of 27 deceased artists. The names of the 27 artists, and the titles of the 40 pictures are given in the table. Four of the 44 pictures which appear in the table could not be used in the construction of Fig. 10 because the dates of their execution are unknown. The four pictures for which dates of execution could not be ascertained are indicated in the table by means of asterisks. Collectively, the 44 paintings that are listed in the table are possibly the world's greatest paintings.

In Fig. 10, as in Figs. 6 to 9 inclusive, the solid line was drawn by tallying each of the 40 oil paintings once only, and the broken line in this figure was drawn by tallying each of the 40 paintings as many times as it had appeared in the sixty lists. Note that each of the curves in Fig. 10 attains its summit at ages 32 to 36 inclusive, and that the peaks of both curves are quite narrow and pointed. It is perhaps no accident that the curve in Fig. 10 which attempts to give greater weight to the very best paintings, namely, the broken line, exhibits a peak which is even more narrow and pointed than is the peak that is formed by the solid line. This latter finding seems to reveal a general principle which will receive further consideration in the paragraphs that follow.

Quality versus Quantity of Production

In preparing their lists of oil paintings some compilers enumerate all or nearly all of a given artist's known works. For example, the compilers of the well-known *Klassiker der Kunst* series present an average of several hundred pictures

for each of a number of artists. For many paintings in the *Klassiker der Kunst* series, the dates of execution are not given by the compilers because the dates of execution are unknown. However, for 30 of the artists whose works are presented in this series, the present writer was able to obtain dates of execution for a total of 7,600 pictures, an average of more than 250 paintings per artist. An age-curve was drawn using these data. The peak of this curve occurred at a relatively late age interval, namely, at ages 50 to 54 inclusive; and this curve tended to sustain itself well at the uppermost age levels.

Other age-curves were then constructed to bring out the relationship between quality of output and quantity of output. Study of many such curves leads to the conclusion that the peak of an age-curve which presents data for paintings of the highest quality is likely to be narrow and to occur prior to age 40. Age-curves which present data for paintings of lesser merit are likely, on the other hand, to reveal somewhat different characteristics. This fact is well illustrated in Fig. 11 which presents data for lists of paintings which vary widely in the degree to which they are selective. For example, in Fig. 11 the solid line sets forth data for the least select of three sets of data, namely, the ages at which 7,032 oil paintings were executed by 135 deceased artists, an average of 52 works per artist (1, 2, 3, 4).[6] The broken line of Fig. 11 sets forth data for 506 pictures by 152 deceased artists, an average of 3.33 works per artist. And the dash line of Fig. 11 presents the one best painting by each of the 61 long-lived artists for whom data were presented in Fig. 9.

It will be noted in Fig. 11 that the curve for the most select list of paintings (the dash line) is relatively narrow; it attains its peak at ages 32 to 36, and it descends rather abruptly after it has attained its peak. In Fig. 11 the curve that was drawn for a somewhat less select list of paintings (the broken line) attains its summit in the forties and the most marked decre-

[6] Dates of execution could not be found for all of the 7,032 paintings. The writer was able to find dates of execution for only 1,053 of the 7,032 paintings, an average of 8.00 works per artist. But the inclusiveness or the exclusiveness of a given list of paintings is to be gauged by the number of paintings per artist—not by the number of paintings for which dates of execution are available.

ment in this curve occurs at ages 42 to 47. Finally, the age-curve for the least select of the three lists of paintings (the solid line) reveals an early rise; it exhibits almost a plateau at ages 32 to 45, and it then falls off very gradually but rises again at the upper-most age levels.[7] In a preceding paragraph it was stated that when an age-curve was drawn by using almost the entire output of a group of artists, *e.g.*, by

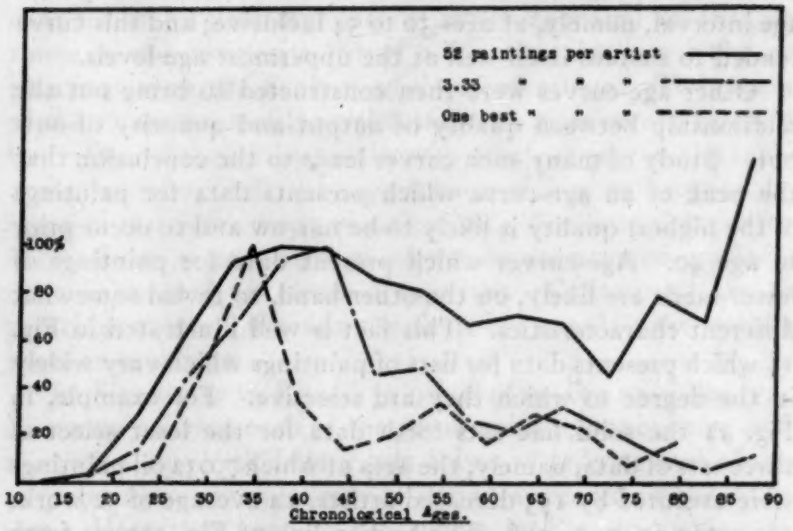

FIG. 11. Curves for lists of paintings which vary widely in the degree to which they are selective. (*A*) 7,082 paintings by 135 artists, an average of 52.07 paintings per artist. (*B*) 506 paintings by 152 artists, an average of 3.33 paintings per artist. (*C*) The one best painting of each of the 61 artists.

employing the data that were obtained from the *Klassiker der Kunst* series, the peak of the curve occurred at ages 50 to 54. It seems reasonable to conclude, therefore, that, in the field of painting, quality of production falls off at an earlier age level than does quantity of production.

It is easy to understand why an age-curve for almost the entire output of a given group of artists rises at a relatively early age level and falls off very slowly. To execute a very large number of paintings, an artist must maintain a fairly

[7] The very marked rise of the solid line in Fig. 11 at the extreme right is probably due to the small number of artists that were alive at these older age levels.

large output over a period of many years. But why do the curves for the most select lists of paintings exhibit their relatively narrow peaks at ages 32 to 36? To the present writer it is a very impressive fact that *more than 25 per cent* of the paintings that were used in the construction of each of Figs. 6 to 10 inclusive were executed during a single five-year interval, namely, from ages 32 to 36.

Concluding Remarks

The shape of a performance age-curve varies with: (1) The type of behavior, (2) the excellence of the performance that is being studied, and (3) the kind of measurement that is employed. This last fact can perhaps best be illustrated by use of an analogy. Thus, one might construct an age-curve setting forth the *average* ability of individuals within each of the various age groups to do the high jump. At almost every age level some individuals would be found who are more or less able to do the high jump. One might, therefore, test out large numbers at each age level and with the resultant data it would be quite possible to construct an age-curve setting forth the *average* height that could be attained by the members of each of the age groups.

But there are several other procedures which might be employed for comparing the several age groups. Thus, within each of the age groups, one might ascertain the per cent of individuals that are able to high jump six feet, the per cent that are able to high jump five feet, and the like. With the obtained data it would then be possible to construct one curve setting forth, for each age group, the per cent of individuals that are able to high jump six feet, another curve setting forth the per cent of individuals that are able to high jump five feet, and so on. If a number of these latter curves were to be constructed, it seems obvious that the curve which set forth age differences in the ability to high jump six feet would start to rise at an older age level and that it would fall off much more rapidly than would another curve which set forth age differences in the ability to jump, say two feet. It is a self-evident fact that very superior high jumping is likely

to occur during a somewhat narrower age range than is the kind of high jumping that elicits no applause.

If we do our thinking not in terms of any hypothetical 'ability' but rather in terms of actual performance, the foregoing situation seems to exist in such diverse fields of endeavor as athletics, mathematics, invention, science, the composition of enduring music, and the writing of great books. For each of these types of behavior, very superior achievement seems most likely to occur during a relatively narrow age-range, and the more noteworthy the performance, the more rapidly does the resultant age-curve descend after it has attained its peak. The findings that have been set forth in the present study with reference to oil paintings and etchings suggest similarly that there is an optimal chronological age level for superlatively great success within these particular fields also. For example, Fig. 11 reveals that an age-curve which is based upon a more select list of oil paintings tends to ascend more slowly and to descend more rapidly than does another age-curve which is based upon a less select list of paintings.

The work of the genius in his old age may still be far superior to the best work that the average man is able to do in his prime. Therefore, for the study of age differences in creativity, it is not valid to compare the achievements of the aged genius with the more youthful accomplishments of the average man. If one wishes to ascertain when men of genius have done their very best work, it is necessary to compare the earlier works of men of genius with their own later works. In the present study this has been done for oil painters and for etchers. The data for architects do not permit such a comparison because most of the architects whose works were studied are still living. It is entirely conceivable that these living architects may do even better work in the future than they have done in the past. Before accepting this highly speculative statement, however, the reader should ponder the following facts with reference to oil paintings.

Figure 12 is based upon 84 modern American paintings by 66 artists, most of whom are still living. Like Fig. 2, which sets forth data for architectural works, Fig. 12 reveals a very

definite peak at ages 40 to 44 inclusive. If the data that are presented in Fig. 12 for oil paintings were the only available data for the ages at which superior oil paintings are executed, it would probably be inferred that oil painters are most likely to execute their best paintings at ages 40 to 44 inclusive. But the collective evidence that has been presented in this article precludes this latter inference. As is shown in Figs.

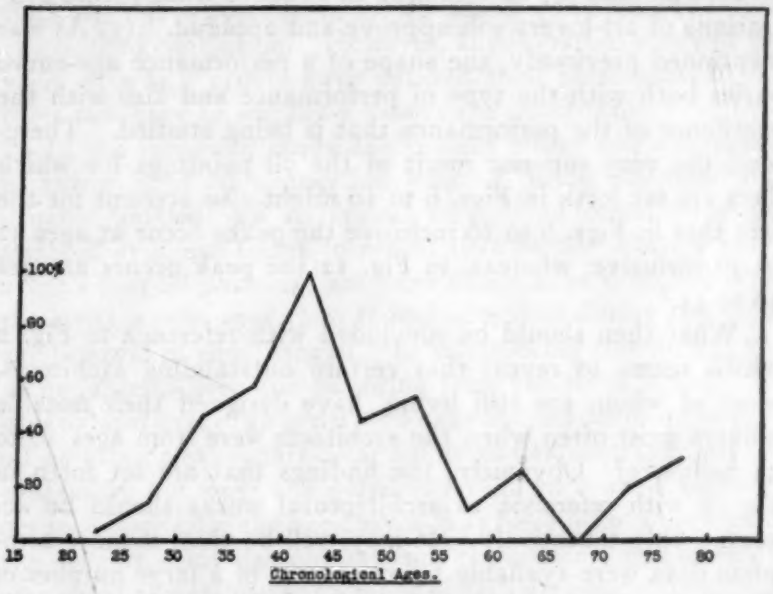

FIG. 12. Modern American paintings. 84 paintings by 66 artists most of whom are still living. Data from Peyton Boswell's *Modern American painting* (5).

6 to 10 inclusive, the best oil paintings are most likely to be executed from ages 32 to 36 inclusive.

How is one to account for the fact that in Fig. 12 the peak of the curve occurs at ages 40 to 44, whereas, in Figs. 6 to 10 inclusive, the peaks occur at ages 32 to 36 inclusive? This difference may be due to any one or more of the following factors. (1) Figure 12 is based upon one book only. As was stated previously, in the field of pictorial art, no one book can be expected to yield wholly trustworthy results. Other books which attempt to present the finest and most significant paintings by living American artists might perhaps yield age-

curves whose peaks would occur at earlier or at later age intervals. (2) Although we may assume that many (or most) contemporary judges would agree with Peyton Boswell's evaluations (see Fig. 12), yet it is still possible that posterity may not evaluate the 84 modern American paintings in precisely the same way that the contemporary critics evaluate them. Tastes in art sometimes change, and there is at present no sure method for determining in advance what future generations of art-lovers will approve and applaud. (3) As was mentioned previously, the shape of a performance age-curve varies both with the type of performance and also with the excellence of the performance that is being studied. Therefore, the very superior merit of the oil paintings for which data are set forth in Figs. 6 to 10 might also account for the fact that in Figs. 6 to 10 inclusive the peaks occur at ages 32 to 36 inclusive, whereas, in Fig. 12 the peak occurs at ages 40 to 44.[8]

What then should be concluded with reference to Fig. 2 which seems to reveal that certain outstanding architects, most of whom are still living, have designed their notable edifices most often when the architects were from ages 40 to 44 inclusive? Obviously, the findings that are set forth in Fig. 2 with reference to architectural works should be accepted only tentatively. It may well be that, if more complete data were available for the works of a large number of deceased architects, those data would reveal that, like the deceased oil painters, the deceased architects would have achieved their greatest architectural works at even younger age levels than are indicated in Fig. 2. Certainly, there is at present no valid reason for concluding that the living architects, whose works are pictured in Fig. 2, will do much better architectural designing in the future than they have done in the past. They, of course, may do their best designing at older age levels, but we cannot be sure of this until more data are at hand.

[8] Figures 6 to 10 inclusive may be analogous to age-curves which set forth, for each age group, the per cent of individuals that are able to high jump six or more feet; Fig. 12, on the other hand, may be analogous to a curve which sets forth the per cent that are able to high jump two, three, or perhaps four feet.

In light of the present findings, it seems evident that many studies heretofore published with reference to man's artistic output have tended to present a rather distorted picture of man's creativity. By use of one or more of the naïve procedures that were mentioned in the first paragraph of this article, namely, (1) merely asserting, (2) quoting mere assertions, (3) the citation of exceptional instances, and (4) 'card-stacking,'[9] it is possible for an investigator to arrive at almost any desired conclusion. However, any impartial study of the years at which artists have painted their most notable pictures will have to employ a more valid procedure.

In the foregoing study the present writer has attempted to throw light on a question which is often discussed but usually 'settled' by an appeal to isolated cases. The assembled evidence seems to reveal that on the whole a given artist's best oil paintings are more likely to be executed when the artist is from ages 32 to 36 inclusive than during any other age interval of equal length. In interpreting this finding the reader should keep the following facts clearly in mind. An isolated instance of an artist who has executed his most notable oil painting at any particular age level demonstrates not the *probability* but only the *possibility* that such an event may occur. Similarly, the age-curves that have been presented in the present study demonstrate not the *certainty* but merely a statistical *probability* that a given artist's greatest oil paintings will be executed while the artist is still in his thirties. Therefore, one is justified only in saying that an artist's one best painting is *somewhat more likely* to be executed from ages 32 to 36 than during any other five-year interval.

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[9] The present study reveals a fifth possible source of error that must needs be reckoned with by those who wish to study age differences in creativity in the field of art, namely, the possibility that certain individual compilers of lists of master paintings have rendered eccentric judgments. Thus, the concensus of *numerous* experts would probably reveal no such rise at the uppermost age level as is to be seen in Fig. 4.

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[MS. received June 8, 1941]

THE RETENTION OF AFFECTIVE EXPERIENCES—A CRITICISM AND RESTATEMENT OF THE PROBLEM

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I. The Studies

It has been some time now since Sigmund Freud advanced his theory that forgetting is not always a passive process, but that in many cases experiences and events are forcefully obliterated from consciousness because they are not in harmony with the ego's ideals, wishes and values. Many experiments have been undertaken with the expressed purpose of proving or disproving the Freudian theory. We may exclude from our discussion the numerous laboratory studies, for as many critics have been quick to suggest, they have probably introduced an artificiality into the situation.[1] As a result, they do not provide as fair a test of the theory as might be possible in a more natural setting. Studies of the retention of experiences from everyday life have perhaps more closely approximated this condition and therefore we turn to them for an evaluation of the Freudian theory.

A study has been reported by Sharp (15) in which the retention of pleasant and unpleasant material was investigated. Sharp defines pleasant material as material which was acceptable to her subjects and unpleasant material as that which was unacceptable. The distinctive feature of Sharp's study is that the material which she used was taken from the clinical case records of her subjects. It was thus possible to test for the retention of material which was definitely known to be acceptable or unacceptable. A comparison of the retention of these two types of material showed that the acceptable was remembered better than the unacceptable.

[1] For comprehensive surveys of these studies, see Meltzer (12), Gilbert (6), and Cason (3).

Jersild (9) has reported a study in which he had his subjects record in a 7 minute period all of their pleasant experiences which they could recall for the three-week period prior to the time of experimentation. He then had them record all of their unpleasant experiences for this same three-week period. At the time of initial recall, the number of pleasant experiences reported greatly outnumbered the unpleasant. Twenty-one days later the subjects were again requested to recall their experiences of the earlier three-week period. Retention of the originally reported experiences was reliably greater for the pleasant than for the unpleasant.

A different approach to the problem has been used by Stagner (16). He had his subjects report only the *most* pleasant and the *most* unpleasant experience which had happened to them during the 15 days prior to the time of the experiment. The subjects were then asked to record all of their associations (*e.g.*, sounds, colors, etc.) with these experiences. Three weeks later the subjects were given a typewritten description of the experiences which they had reported at the first recall, but with the associations omitted. When the subjects were asked to reproduce as many of the original associations as possible, the percentage of recalled associations was reliably larger for the pleasant experiences than for the unpleasant experiences.

Meltzer (13) asked college students to describe all of their experiences which had occurred during Christmas vacation period on the day following Christmas vacation. The students were then asked to rate their experiences as pleasant or unpleasant. Of the experiences reported, more were rated as pleasant than unpleasant. Six weeks later, the subjects were again asked to recall their experiences of the Christmas vacation period. At this time the average per cent of the pleasant experiences recalled was greater than that of the unpleasant.

Waters and Leeper (17) repeated Meltzer's study with but a slight alteration of procedure. They also had their subjects rate experiences which had happened during Christmas vacation period on an affective scale. They found—as Meltzer had—that more of the experiences reported were

rated as pleasant than unpleasant. After the final recall, they also had the subjects estimate the number of times they had thought about or mentioned these experiences since the time of their occurrence. They thus had a crude measure of the relative amount of review accorded the pleasant and unpleasant experiences. In comparing the retention and rehearsal of the pleasant and unpleasant experiences, Waters and Leeper found that both the extremely pleasant and extremely unpleasant experiences were remembered better and reviewed more often than the indifferent experiences or those which were only slightly pleasant or unpleasant.

Koch (10) has reported an interesting approach to the problem. She had her subjects rate the affective tone of their grades on each of 10 objective examinations. Five weeks after the last objective test was given, the subjects were asked to recall all of the examination grades which they had received. The results showed that those grades which were rated as pleasant tended to be recalled better than any of the others. Koch also found that intensity of affective tone was a factor in retention, for both the very pleasant and the very unpleasant grades were recalled better than those which were rated as neutral or only slightly pleasant or unpleasant.

The findings of the studies which we have mentioned may perhaps be summarized as follows: First, at the initial recall period a predominant number of pleasant experiences are reported. Second, at subsequent recall periods both extremely pleasant and unpleasant experiences tend to be remembered better than those which are neutral. Third, of the two extremes (pleasant and unpleasant), the pleasant experiences tend to be remembered better than the unpleasant.

II. The Theories

Since the basic theory underlying the experiments reported seems to be that of Sigmund Freud, a consideration of his point of view will serve to orient us with respect to the various alternative or supplementary theories which have been advanced by these investigators in attempts to account for their findings.

The Freudian theory may be stated in terms of repression. Repression is the exclusion of material or experiences from consciousness and emphasizes an 'active' process of forgetting rather than a 'passive' process which is merely correlated with time. Significance is attached to the role played by an organism's desires, beliefs, attitudes, etc., in determining what is forgotten and what is remembered. The function of repression is to prevent the development of disturbing emotions by protecting the individual from memories which might arouse them. Thus experiences which are not in harmony with the desires, wishes, beliefs, etc., of an individual would tend to be repressed.[2] Sharp's data could be accounted for in these terms.

Many investigators, however, have been prone to deny the Freudian theory as it has just been stated and have offered various substitutes. Thus, Jersild (9) does not believe that unpleasant experiences are forgotten because of active repression but because of a rehearsal differential. Unpleasant events tend to be forgotten because there is less rehearsal of them than of pleasant events, *i.e.*, we tend to repeat to ourselves and to others the pleasant things which have happened to us, whereas we seldom mention the unpleasant things. The study by Waters and Leeper (17), however, which employed ratings of the relative amount of review given to pleasant and unpleasant experiences, throws some doubt upon Jersild's unsupported explanation in terms of differences in frequency of rehearsal.

Jersild also advances a second factor to account for the forgetting of unpleasant experiences. He believes that they will tend to be forgotten because with the passage of time they lose their emotional tone. When an individual encounters an unpleasant situation, certain steps are taken to modify that situation. "The activity pattern of which the unpleasantness was originally a part frequently terminates in alleviation or relief." As a result "memory of many un-

[2] See Healy, Bronner, and Bowers (8) for a discussion of the concept of repression in psychoanalytical theory.

pleasant events will be modified in terms of activities undertaken to relieve the unpleasantness" (9, pp. 287–288).

Stagner (16) has elaborated the above theory and restated it in terms of 'retroactive inhibition.' Stagner reports that an examination of the unpleasant experiences reported by his subjects shows that most of them involve the setting up of some kind of tension (*e.g.*, receiving a notice from the Dean), while the pleasant experiences involve a release of tension (*e.g.*, finding a sum of money which has been misplaced). According to Stagner, the unpleasant experiences represent situations which are not complete, in which the response is not adequate, and about which the individual must do something. Consequently, he tries other modes of response. In contrast to this, the pleasant experiences represent situations which are complete, in which the response is adequate, and about which the individual need not do anything.

Considered in this fashion, unpleasant experiences always evoke inadequate responses which are in turn followed by other responses. Unpleasant experiences are thus more subject to the influence of retroactive inhibition.[3] Pleasant experiences are less influenced by retroactive inhibition because the responses to the situations which arouse pleasant feeling tone tend to remain the same or equivalent.

A serious objection to this theory is found in the results of a study reported by Zeigarnik (19). In brief, her experimental technique involved having children work at certain interesting tasks. They were allowed to finish some of the tasks but were interrupted before they had finished with others. Some of the activities were thus completed and some were not. Accordingly, the interrupted tasks should represent situations which were not complete, which involved

[3] We do not disagree with Stagner's statement that unpleasant experiences may result in tension, which in turn causes a quantitative increase in the general level of activity. Some writers consider this to be the important variable in determining the degree of retroaction. An alternative point of view, however, stresses the fact that retroaction is a qualitative as well as a quantitative phenomenon. The proponents of this point of view believe that the degree of retroaction is related to the similarity of the subsequent activity to the original—within limits of course. According to this latter theory of retroaction, even though unpleasant experiences may involve tension which raises the level of activity, retroactive inhibition may not necessarily follow.

tension, and in which the response was not adequate. They should, therefore, be followed by retroactive inhibition and hence be forgotten. However, when the subjects were asked to recall the tasks upon which they had been working, memory was consistently found to be better for the interrupted tasks than for the completed.

Menzies (14) has advanced the theory that the retention of affective experiences is correlated with the intensity of feeling tone rather than the quality. Waters and Leeper (17), in an independent investigation, reach the same conclusion. It is difficult to accept this intensity theory, in the face of other facts.[4] The majority of studies do report a qualitative difference in retention. Moreover, there is evidence that this qualitative difference is present even when the experiences reported are relatively comparable as far as intensity is concerned. Thus in the study reported by Stagner (16), subjects recorded only the *most* pleasant and the *most* unpleasant experience. This would suggest that the intensities of the pleasant and unpleasant experiences were fairly comparable and that such differences as might exist would tend to be balanced out for the group as a whole. Stagner's results would seem to show that even where intensity is to some extent controlled, a retention differential is found.

The various theories which have been discussed may be classified under two general headings: those which accept a differential in retention of pleasant and unpleasant experiences and those which do not. The findings of Meltzer (13), Sharp (15), Jersild (9), Koch (10), and Stagner (16) support a retention differential, while those of Menzies (14) and Waters and Leeper (17) do not. The latter investigators, failing to find a retention differential, postulate that the intensity of affective tone is sufficient to account for retention and forgetting of affective experiences. However, Meltzer, Sharp,

[4] We do not mean that intensity is not *a* factor in retention. We are here objecting to the *intensity theory* of these investigators. While the theory may adequately account for the findings of their studies, it fails to explain certain facts (*e.g.*, clinicians have reported many cases where intense affective experiences have—for all practical purposes—been forgotten).

Jersild, and others, having found a difference in the retention of pleasant and unpleasant experiences, are faced with the task of explaining their results on the basis of some qualitative difference. They have thus variously proposed: (*a*) that unpleasant experiences are actively suppressed while pleasant ones are not; (*b*) that unpleasant experiences are subject to the influence of retroactive inhibition while pleasant ones are not; (*c*) that unpleasant experiences are rehearsed less frequently than pleasant ones. We have already raised objections to (*b*) and (*c*) above. We now propose to advance a more general criticism, which is perhaps applicable to any interpretation of the Freudian theory purely in terms of affective tone.

III. Restatement of the Problem

As we have pointed out elsewhere, "the soundness of any position regarding the Freudian theory of memory in terms of pleasantness and unpleasantness depends of course upon the correctness of the assumptions which it makes concerning the relationship between pleasantness and unpleasantness and the ego's desires, beliefs, attitudes, etc." (4, p. 34). It is commonly assumed by those investigators who seek to test the Freudian theory by relating the quality of affective tone to forgetting and retention, that all pleasant experiences are in harmony with the ego's desires, while all unpleasant experiences are in conflict with them. It is our opinion that these relationships do not necessarily hold in all cases. Pleasant experiences, for example, may actually conflict with a specific frame of reference. In our own culture this is probably true with respect to certain aspects of sexual behavior. In fact, theoretically, either pleasant or unpleasant experiences may be related to a frame of reference in at least four ways: (*a*) they may harmonize with the frame; (*b*) they may conflict with the frame; (*c*) they may not involve, in any great degree, a frame at all; (*d*) in the case of non-ego involved frames (such as familiar perceptual-motor frames) they may be indifferent or neutral. The important consideration, as we see it, is not so much a matter of determining whether an ex-

perience is pleasant or unpleasant, but rather a matter of determining the presence or absence of conflict of this particular experience in terms of the individual's values. In other words, is the experience compatible or incompatible with the frame of reference of the individual? The question of the affective tone of the experience is secondary to the question of conflict and not necessarily basic.

The divergent results of some of the investigations of affective tone as related to retention may be due in part to the failure of the investigator to ascertain the relationship between the experiences reported by the subjects and their frames of reference. Instead of placing emphasis upon the affective tone of the experiences and then assuming this relationship to be indicative of conflict, we suggest that it might be more valuable to determine the presence or absence of conflict by another means. As we have tried to show, this resolves itself to a determination of the frames of reference of the subjects. Knowing this variable, we can then proceed to predict fairly accurately which experiences will tend to be forgotten and which will tend to be remembered.[5]

Observations of everyday behavior furnish us with illustrative cases. Brown is an ardent advocate of the labor movement and he tends to learn, remember, and emphasize in his thinking those aspects of the labor movement which are in harmony with his frame of reference. In his discussions of labor he fails to recall any of the undesirable characteristics which are sometimes associated with labor unions. Similarly, Smith, who is a staunch Republican, has great difficulty in recalling anything which presents the New Deal in a favorable light. Yet he experiences little trouble in remembering the taxes levied on his business, the 'court packing bill,' and other 'attacks' on his business which were sponsored by the New Deal.[6] In either case, the things which are learned and

[5] *Cf.* Edwards (4) and Watson and Hartmann (18).

[6] This is an instance in which unpleasant experiences are remembered rather than forgotten, because they are consistent with the frame of reference concerning attitude toward the New Deal.

remembered tend to harmonize with the frame of reference of the particular individual.[7]

Experimentally, there is also evidence for the theory as we have stated it. Watson and Hartmann (18) compared the recall of atheistic and theistic material for subjects with atheistic and theistic attitudes. They found that material which was consistent with the attitudes of their subjects was recalled much more effectively than material which was opposed to their attitudes. Edwards (4) has compared the retention of Pro and Anti New Deal material for subjects with Pro and Anti New Deal attitudes. Retention in all cases was significantly greater for material which was compatible with the attitudes of the subjects than for material which was incompatible.[8]

In addition to giving an advantage in learning and retention of material and experiences which are consistent with it, there is evidence to indicate that the frame of reference is also a factor in the recasting of material which is not readily assimilated, so that the material will more readily fit in with the frame. Bartlett's (1) classical study of memory illustrates the point. He found that the frames of reference of his subjects definitely influenced their reproductions of simple stories. Unfamiliar parts of the story were eliminated or recast into incidents which were more familiar. Similarly, Edwards (5) has shown that one's attitude is influential in distorting the recognition of material which is inconsistent with it in such a manner as to minimize conflict.

[7] Perhaps we have here a new insight into the problem of why "what is" tends to continue to be; *i.e.*, why there is so little deviation from accepted social norms. The shift from an accepted point of view to one which is critical of the accepted, involves the necessity of 'forcing' oneself to learn that which is contrary to what one believes in the first instance.

[8] There are several other sources which bear indirectly on the problem. Lund (11) has demonstrated that if one wishes a group to remember certain ideas presented to them, one should first convince the group that the ideas are in accord with those which they already have. Biddle (2) has shown that it is possible to have 'false' conclusions accepted by a group simply because they seem to fit in with existing preconceptions of the group. Hartmann has established the fact "that political propaganda is influential only to the extent that it is an expression of the present desires, sentiments, prejudices, interests, limitations and properties of those to whom it is addressed" (7, p. 98).

It should be emphasized at this time that the chances are relatively unfavorable that the frame of reference will be seriously altered rather than the conflicting material or experience. The numerous divergent studies of the influence of the classroom, movies, lectures, etc., as related to change in *established* frames of reference, leave so many factors unaccounted for—and even then show such meager shifts when they do occur—that they raise serious doubt as to the extent to which modification occurs, at least in these situations. This is not the same as saying that frames of reference, once established, never undergo change. As a matter of fact they do. But as to 'how' or 'why' is another problem and not within the scope of the present paper. It is sufficient for our purpose here to point out that an examination of experimental attempts to provoke reorganization of established frames of reference seems to indicate that drastic and permanent change is the exception rather than the rule.

IV. Summary

In summary, we may briefly restate the various properties of the frame of reference as we believe they are related to learning and retention. We have held: (*a*) that experiences which are consistent and which harmonize with an existing frame of reference will tend to be learned and remembered better than experiences which conflict with the frame; (*b*) that experiences or materials which are in opposition to the frame of reference will tend to be recast so that they may be more readily assimilated; (*c*) that because of the relative stability of the frame of reference, the chances are slight that the conflicting experiences will cause a serious reorganization of the frame itself.

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[MS. received June 6, 1941]

VALID AND INVALID CONCEPTIONS OF OPERATIONISM IN PSYCHOLOGY

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I. Introduction

G. Udny Yule's work is one of the classical sources of the practice of exact measurement in psychology, but his warning in regard to the thirst for exactness at all costs is not heeded. He writes, "Failing the possibility of your measuring that which you desire, the lust for measurement may, for example, merely result in your measuring something else—and perhaps forgetting the difference—or in your ignoring some things merely because they cannot be measured" (**9**, p. 107).

Operationism appeals to psychologists because it promises to give the same rigor to the definition of concepts which mathematics provides in treating experimental data. But, if operationism is not carefully defined, it will yield an abortive result similar to the one referred to in the citation from Yule. If operations are defined as the procedures *resulting* in the establishment of a systematic set of concepts, we have a conception of it which will be fruitful in psychology. We must beware of the disasterous results of adopting the tempting assertion that the operation is *identical* with the concept.

It will be well to give an account of the *motives* which led to the adoption of operationism on the part of P. W. Bridgman, the famous champion of this point of view. In his *The Logic of Modern Physics* (**2**) Bridgman makes it clear that his adoption of operationism was a result of the advent of relativity and quantum theory. It was a great 'shock,' Bridgman says, to discover that the conceptions of classical physics were so out of harmony with the facts of nature. Looking to the future of science, Bridgman anticipates that our views of nature must always be subject to change; but

"there is a part of our attitude to nature which should not be subject to future change, namely that part which rests on the permanent basis of the character of our minds. It is precisely here, in an improved understanding of our mental relations to nature, that the permanent contribution of relativity is to be found. We should now make it our business to understand so thoroughly the character of our permanent mental relations to nature that another change in attitude, such as that due to Einstein, shall be forever impossible" (2, pp. 1–2).

Operationism is the point of view which Bridgman offers to prevent the 'shocks' which confront classical physics in the form of relativity. Operationism is that 'improved understanding of our mental nature' which need not be subject to future change. Bridgman defines operationism as follows: "To find the length of an object, we have to perform certain physical operations. The concept of length is therefore fixed when the operations by which length is measured are fixed: That is, the concept of length involves as much as and nothing more than the set of operations by which length is determined. In general, we mean by any concept nothing more than a set of operations; *the concept is synonymous with the corresponding set of operations*" (2, p. 5).

I wish first to call attention to the fact that Bridgman reveals more frankly than do the psychologists the *motives* which underlie the adoption of operationism. The appeal of operationism is due to its strategical value for avoiding *ontological questions.* For, if at any future date nature fails to show the attributes implied by our concepts, we can say that in fact the concepts refer, not to nature, but to our operations on nature. If electricity is defined as that which the galvanometer measures, we are relieved of the embarrassing task of deciding what electricity is in its own nature. For the psychologist, if intelligence is defined as that which the intelligence test measures, a similar Gordian know is cut. If the concept refers to the operation and not to nature, physics may ignore the question of what the ultimate nature of matter is like. Since human beings, the chief subject matter for

psychologists, are at once body and mind, psychologists make a double gain by adopting operationism as defined above; for it justifies them in ignoring ontological questions regarding both body and mind.

I wish next to call attention to two ambiguities which appear in Bridgman's definition of operationism. The more ordinary theory of knowledge is triadic: that is to say, we assume three things, the object (nature), our operations on the object, and the concepts resulting from the operations. A clear-cut operationism would reduce this triadic relation to a dyadic one: We may ignore the object, and concern ourselves only with operations and concepts. I do not say that the operationist must declare that objects do not exist, but only that the object may be ignored in the pursuit of knowledge; for operationism of the dyadic sort maintains that the operations determine our knowledge (concepts) completely. And yet, Bridgman does not say that nature may be ignored. In a final chapter of his book (**2**, Chapter IV) he discusses 'Special views of nature'—the question of her simplicity, determinism, and other rather abstract characteristics. His views, therefore, regarding the role played by nature in our pursuit of knowledge are ambiguous and hazy.

A second and parallel ambiguity appears in his definition of operationism itself. Note the two contradictory conceptions of operationism which he gives in a single sentence, quoted above. A concept means 'nothing more than a set of operations,' and yet "*the concept is synonymous with the corresponding set of operations.*" If the concept means the same thing as a set of operations, then nature might be ignored entirely in our pursuit of knowledge. But Bridgman assures us, in the same breath, that the operation and the concept are only *synonymous.* Now, synonymous meanings are those which are *nearly the same.* The situation seems to be that we can ignore nature, but not entirely, because the operation does not quite determine all of the concept; or, what is worse, nature determines the character of the operation which we must use. In short, Bridgman appears to be seeking a point of view which will deliver us from the shocks

which nature gives us by failing to conform to our concepts, and yet he wishes to believe that our concepts reveal the true attributes of nature.

The fundamental difficulty in Bridgman's views is whether operationism will satisfy the motive for adopting it which has activated him. He adopted operationism to avoid the 'shocks' which nature inflicts on us by failing to conform to our cherished concepts. My criticism is that if operationism is adopted, these 'shocks' merely pass into the 'shock-absorber.' That is to say, perhaps many physicists love their operations more than they love nature; and it will be shocking to find a new operation which entirely discredits an old one! Poincaré says somewhere that science sometimes clarifies nature by shutting up all of the darkness within itself! Bridgman's solution seems to me to transfer the contradictions which arise between concepts and nature to contradictions which arise between various operations. The hiatus between the concepts of relativity and those of classical physics is reflected by a similar hiatus between the methods of relativism and those of classical physics.

Before leaving Bridgman, I wish to note one way in which he uses operationism which seems to me to be excellent. This occurs in his discussion of the measurement of distance (**2**, pp. 9–18). When we measure the length of a room there are grounds for believing that we arrive at a concept which reveals the true attributes of the room, *i.e.*, its spatiality. The space of the room is gauged in terms of multiples of a ruler which also has the attribute of spatiality. The distance of the moon, however, cannot be measured directly. This distance could be determined by triangulation from two widely separated points on the earth; but now *light* plays an important role in the operation. To measure the distances of points outside the solar system we resort to measures of parallax, of light years, of shifts in spectra. At this point, we are not sure that we are measuring space at all! It is therefore a wise precaution to say that the distances of such points have reference to the methods by which they were measured. This is merely to warn us of the possibility that the operation did

not measure space in the ordinary sense at all. This narrower use of operationism is excellent; but Bridgman, with a sweeping thirst for generalization which scientists unknown to themselves share with the philosophers, universalizes the principle, and claims that operationism applies as well to the original measurement of the room.

II. Operationism in Psychology

My chief purpose, however, is not to criticize Bridgman but to examine the use of operationism on the part of psychologists. Usually, psychologists define operationism in less hazy fashion than does Bridgman. McGeoch proposes "that learning can be more adequately defined in terms of the operations of measurement than in terms of phenomenal properties" (5, p. 688). McGregor defends operationism defined as "the belief that an entity is adequately defined only in terms of the specific operations involved in its observation" (6, p. 247).

The psychologist's use of operationism to escape the entanglements of ontology may be illustrated by several examples. Consider Boring's treatment of our experience of duration and of temporal patterns. These experiences are notoriously subjective and inconsistent with clock-time. It would be a blessing if we could rid these experiences of their subjectivity and escape the need of characterizing them as mental processes. Boring accomplishes these results by adopting an operational definition of duration as follows: "A perceived duration or temporal pattern is defined by the operation of introspective report by means of which it is studied. It need not be regarded as an entity, but as a public and verifiable construct. The concept of duration is an *inference* from introspective reports: duration as an immediate experience is meaningless" (1, p. 521).

Another example is from Stevens who maintains that for both physics and psychology the act of *discrimination* is fundamental. Discrimination is an operation which denotes or points to something. Equality means the absence of discriminable differences. When operations fail to reveal a dis-

criminable difference, we have equality. Thus, we do not ask whether objects or processes are equal in themselves—equality refers to the operation of discrimination, not to the objects discriminated (7, pp. 324–325).

We have seen how Bridgman invokes operationism to meet the difficulty raised by relativity regarding the existence of an independent realm of objects. In the physical world, an object no longer has an absolute existence of its own. Its traits arise largely if not entirely from other objects which surround it. A given body has mass only because other bodies around it exert gravitational pull on it. An electron appears more and more to be merely a nexus of relations sustained between other bodies which surround it, which in their turn are described in the same way. In place of Aristotle's dictum that *everything is what it is* we are led to say that *everything is something else!* Consider how this relativity of things bedevils the psychologist who wishes to show that there are mental processes which have a circumscribed and rightful existence of their own. Carr, for example, has examined the merits of learning things by wholes or learning them part by part. He concludes that the method which is the more efficient varies with each variation of the situation: The age, training, memory span, I. Q. level of the subject, the form of the material learned, the method of learning it, etc. These facts suggest to Carr the advisability of adopting a relativistic point of view in psychology—that the results are relative to the methods and conditions used in attaining the results (4, pp. 521–522).

It will be well to begin my criticism of operationism in psychology by noting certain objections to it which seem to me to be invalid. The most obvious of these is to reject operationism because of our habitual and emotional attachment to the time-honored triadic view that a theory of knowledge must assume three things: a realm of objects, operations made on these objects, and concepts resulting from the operations. For instance, it will not do to say that operationism is invalid because operations are only *adequate* or *inadequate*, not *true* or *false*: Only concepts may be true or false, since

they alone refer to nature. We have here only the assertatory acceptance of the triadic theory. The objector may argue that the entire historical structure of inductive logic and empirical science assumes the existence of objects which determine what the nature of concepts must be. Of course, it may be the very purpose of operationism to break down this historical view, and we cannot allow the mere attempt to be regarded as proof of its own invalidity. The length of time during which a point of view is accepted creates a presumption of its truth but does not establish its certainty. If the invalidity of the dyadic view is to be established, we cannot use data from the history of science if this history shows no serious attempts to use it.

A second objection consists in saying that concepts contain much more than operations can yield. A physical instance of this objection would be to say that our concept of the room's length is not entirely given by the technique of measurement. The operation with the ruler yields a number, but that this number refers to *length* is not given by merely counting. Surely the room had length before we measured it. The ruler gives us discontinuous units, but we believe that the length of the room is continuous. The same Bergsonian objection may be made regarding measurements of time. This argument invokes Aristotle's distinction between the measured and the measuring time.

I will cite a similar instance of this type of objection from psychology, namely, in regard to color contrast. The purpose of the objection is again to show that the concept contains more than is indicated by the corresponding operation. Color contrast appears whenever I view any surface whose contours are hazy. Here it seems obvious that the operation determines only the *conditions* under which contrast appears. The contrast *experienced* is not in the stimulus conditions at all, but in the observer.

Operationism, however, may reply to all this by saying that the objections hold because operations are defined too narrowly. Admit that we know what the length of the room is like because we *traverse* as well as *measure* it, but the

traversal of space is but another operation. The belief that the room is continuous may be regarded as the result of rational operations rather than with operations with the ruler. Thus, Zeno's paradoxes are logical arguments (operations) refuting the notion that space could be composed of an infinity of spaceless points. No doubt we need more than laboratory equipment to demonstrate color-contrast; we need an observer who adds an essential operation of his own. The color-blind man will not see color-contrast—he is incapable of the necessary operation.

The critic of operationism may declare that the realm of immediate experience lies beyond the sphere of operations. The term 'operation' seems an inadequate designation for experiences so immediate as the seeing of a color or the hearing of a tone as it is experienced inwardly. This is the reason why Boring rejects immediate experience as a psychological source of scientific data. He wishes to confine science to mediate operations because only these are so to speak 'public.' The material sciences too must infer the nature of their objects from public and verifiable observations, which also have their origin in 'immediate experiences.' Psychology is in no greater difficulty in this respect than any other science.

Another charge against operationism is that it would inevitably lead to the uncritical multiplication of methods. Waters and Pennington write, "The slightest change in any aspect of a set of operations would mean . . . a new concept, and would demand, likewise, a new symbol for its designation" (8, p. 418).

It is true that Bridgman demands that each set of operations be a *unique* set. But the operationist may reply that *common denominators of method* will be found to exist, and that these common elements will be reflected in the resulting concepts. Thus, in physics, the specific heat of a gas may be determined by the direct method of literally heating the gas and measuring the rise in temperature. Or, the speed of sound may be determined in the gas before and after heating it. Calculations from both sets of data will yield the same measure of specific heat for a given gas. In psychology, color-

contrast may be studied with colored papers and tissue paper, with a color motor, by viewing colors in twilight, by viewing colors at a distance. In all four of these methods the common operational feature is that color contrast appears whenever the contours of the stimuli are obliterated or obscured. In this case, it does not follow that the adoption of operationism will lead to an inordinate multiplication of concepts or operations; and I think that the same may be demonstrated for other fields of research in psychology.

I arrive now at the crucial objection which I would urge against the form of operationism which identifies the operation and the concept, and seeks by this means to escape ontological questions. Our conviction regarding the existence of a realm of physical objects and of mental processes that lie *beyond* the operations, arises from a certain *necessity* or *requiredness* which they impose on us. When the traditional scientist attained the same results by quite different methods he was doubly sure that he had discovered the true lineaments of nature. Why? Because of this evidence of the existence of a realm of brute fact which determines (1) what methods we must use, and (2) what the results will be. Thus, we cannot choose to measure electricity with a gas meter. Instead of freely selecting instruments with which to operate on nature, we must perforce select instruments on which nature will operate. Now, my point is that operationism cannot ignore this external realm of necessity, for all of the necessity will reappear in the realm of operations. In the case of measuring the specific heat of a gas, it makes no difference as far as *requiredness* is concerned whether we say that there is some ontological necessity which makes the sound conductivity of a gas vary with its heat; or whether we say with the operationists that there is a necessary connection between two ways of measuring heat. The traditional physicist was led to believe in the existence of a realm of reality consisting in what Professor Smart calls 'the systematic interconnection of things.' The operationist will encounter the same degree of systematic interconnection among his operations.

It is only when the operationist forgets this aspect of necessity that we may fear that the multiplication of methods will run riot. Forget the mutual implications existing between methods, and each method is as good as any other. When an astigmatic person looks at an object he may see several overlapping objects. This is interesting as a consequence of the operation by means of which an astigmatic person sees things. But, if one adopts the brand of operationism which ignores the necessary interconnection between operations, the consequence would be that the normal man who sees but one object is no more right than the astigmatic subject: the concept is a function of the method! Of course, the operationist knows this full well, and also realizes that a proper understanding of the properties of lenses will explain why the normal man sees one object and the astigmatic man several of them. But this is to grant that there is necessity and systematic interconnectedness in the realm of operations no less than in the realm of nature. What then has been gained by adopting operationism? It has been adopted to escape the need of characterizing the ontological nature of physical objects and of mental processes. The ontological nature of things may be a nuisance, but they are no less so when entrenched in method than when they are entrenched in nature.

III. Operationism and Logical Positivism

The common man believes that *length* belongs *attributively* to the room and not to the operation, because it will be of no avail to measure the room in inches rather than feet in the hope of proving it larger. The operationist comes to the same result; for further operations on inches and feet will show the *necessity* of having twelve of the first to make one of the second.

Sometimes psychologists introduce their books with a most emphatic rejection of all philosophy, and then proceed to list a set of metaphysical statements abstract enough to render Descartes green with envy. Titchener is said to have observed that behaviorism is a consequence of the ignorance

of history. This ignorance of history is shown by operationists in psychology who are in close kinship with the logical positivists in philosophy. I will trace this resemblance briefly in the case of Rudolph Carnap (3).

Logical positivists manifest the desire we have noted in operationists to dodge ontological questions. Carnap proposes to achieve this by translating all predicative statements into syntactical statements. Syntactical statements have to do only with the *form* of the assertion, not with its *material content.* Thus, the material mode of speech: "The rose is a thing," becomes, "The word 'rose' is a thing-designation." In the same fashion material statements regarding relation, number, causality, modality, may be translated into syntactical form.

Carnap's specific purpose is to achieve a systematic position which will enable the philosopher to dodge the question of what things really are. He declares that if we do not avoid the material mode of speech "we shall find ourselves sliding into the midst of metaphysics—and that is sliding into the mud" (3, pp. 95–96).

This position is far more defensible for a philosopher than it is for a psychologist. In a true sense, questions of the existence and nature of 'things' (whether these are physical or mental entities) *are* the concerns of science, not of philosophy. Carnap is seeking to establish a clear-cut sphere of activity for philosophers. He conceives this to be the analysis of the languages of the sciences. There is no philosophy of nature, but only the philosophy of natural science; no philosophy of biology, but only the philosophy of biological science, and so on for all of the sciences.

In this same fashion operationists might conceive it to be their task to make a systematic study of the methods of science *as such.* But we have seen that this is not at all their intention. Carnap, however, after giving such wise council, proceeds to set a bad example. This is shown in his defense of *physicalism,* a doctrine for which Dr. Neurath is particularly responsible. Physicalism maintains that physical language is the basic language of all science. That is to say,

every sentence of any branch of science can be translated into physical language. I call Carnap's example a 'bad' one, because after rejecting all ontological questions, he inconsistently accepts physical statements as the basis for all other statements. I will illustrate his manner of translating psychological statements in particular into physical form. The reader will recognize the familiar devices of behaviorism.

Carnap gives the following example: He says that the psychological sentence, "At ten o'clock Mr. *A.* was angry," may be translated into the physical sentence, "At ten o'clock Mr. *A.* was in a certain bodily condition which is characterized by the acceleration of breathing and pulsation, by the tension of certain muscles, by the tendency to certain violent behavior, and so on" (3, pp. 89–90). With the help of a scientific law, says Carnap, we may reverse the last statement and say that whenever Mr. *A.* is in the bodily state mentioned he is angry. Walter B. Cannon, in fact, would reject the possibility of this simple conversion, for his researches fail to show that there is a one-to-one relation between *felt* emotions and their bodily accompaniments. This, of course, does not invalidate Carnap's proposal in general; for, if Cannon is right, there is no scientific law which justifies the translation. Carnap clearly wishes to make such translations only when they are justified empirically. If the mental state has no organic accompaniment, Carnap maintains that such mental states cannot be verified at all because they are not *observable*. Mr. *A.* may give a report concerning his *felt* anger, but Carnap notes that such reports *are* observable.

In the terminology of logical syntax, statements which have the same meaning are called 'equipollent'; and Carnap regards the physical translation in the case of Mr. *A.* as equipollent to the psychological statement that Mr. *A.* is angry. If we give consent to the facts, when a subject is angry he both *feels* anger and displays certain organic states. Which one of these is really the essential part of anger? This is an ontological question which Carnap advises us to dodge, but his own adoption of physicalism shows that he prefers the mud of metaphysics after all—he cannot resist

the conviction that physical realities (the angry man's bodily processes) are more 'fundamental' than his mental states. The chief source of comfort, however, is to get rid of at least one of two realities, which appear to have nothing in common; for what does felt anger have in common with a rapid pulse, muscular activity, etc.? We are not surprised to see a philosopher succumb to the love of monism; but psychologists, as can be shown, are subject to the same temptation.

Operationally considered, the method by which the subject learns of the existence of *felt* anger is radically different from the physiologist's method of studying it. Like Carnap, behaviorists quite generally throw the introspective method overboard and end up with the ontological assertion that the bodily state *is* the emotion. This is done merely because the physiological approach is regarded as more 'objective.' It is clear that the adoption of operationism may fail entirely to free the psychologist from ontological concerns. In the case of Carnap and of behaviorism, ontological interests may be denied, but they succeed nevertheless in determining the type of operations which will be accepted. This arbitrary violence in the realm of scientific method is not so likely to occur if the operationist will remember that there is a realm of realities which determine what his choice of operations must be. If two equally valid operations, for instance introspective and physiological ones, point to the existence of two radically different realities, let us for the present accept the facts rather than yield to the philosophical desire for monism. I should like to cite a statement from Bridgman which it is all too common to skip over. "The fact has always been for the physicist the one ultimate thing from which there is no appeal, and in the face of which the only possible attitude is a humility almost religious" (3, p. 27). Often, the only way to achieve monism is by doing that violence to facts which consists in neglecting their differences. Ernst Mach somewhere observes that if we are willing to ignore differences, a penny and the moon may be regarded as identical. This is the only way, so far, in which monism has been achieved in psychology.

This is the only way in which operations may be made identical with their corresponding concepts.

IV. Conclusions

I conclude that the object of knowledge cannot be incorporated in the operations by means of which we know it. There is a realm of brute requiredness which cannot be 'liquidated' by the simple device of identifying operations and concepts. Operationism of this sort may ignore the systematic order or lack of it in the things upon which operations are made, but the systematic order or lack of it will reappear amongst the operations. The 'shocks' which we experience when we see that concepts fail to fit the realities are indeed precious experiences which keep science going. When we note such inconsistencies we renew our efforts to restore system. We are always under the necessity of assuming that the interlocking system is there, and that it is our business to find it. If, on the other hand, we regard the concept as referring, not to the fact, but only to the operation, far from attaining systematic science, we attain a welter of inconsistent methodologies. Carnap's syntactical approach results in a welter of mutally inconsistent languages. The dense fog of Hegelianism resulted from the initial assumption that reality is identical with concepts (ideas). The assumption that operations are identical with concepts has all of the ingredients necessary for a similar fruition of metaphysical verbiage. The psychologist who adopts operationism will do well to regard the concept as derived *by means of* the operation. As various methods for measuring a given reality are perfected they will tend to yield the same concepts. Thus concepts gradually become more and more independent of any one method, and conform more and more to the nature of reality whose existence is also independent of method. However, *when concepts are new, we must take special care to remember their derivation from and dependence on the methods by which they were secured.* This simple and apparently obvious warning embodies the chief value of the recent advent of operationism in psychology. The warning is not superflu-

ous, for numerous and futile debates have their origin in the failure to realize that contradictory findings are conditioned by the methods of securing them. A sound operationism thus accepts a triadic view of knowledge, and concerns itself with the intimate interdependence between two members of the triad, namely, between operations and concepts.

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[MS. received July 21, 1941]

CAN INFANTS HAVE IQ'S?

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The attempt of the last decade to measure the intelligence of young infants constitutes perhaps the least satisfactory achievement in the entire area of child psychology. In spite of accumulations of data and the multiplication of tests, the intelligence of the baby presents an almost insurmountable barrier to known techniques for its measurement. The purpose of this article is to suggest some reasons for the unsatisfactory status of this effort. In order to do this an attempt will be made to analyze the physiological and behavioral conditions capable of supporting the type of behavior to which the term intelligent may be applied. No special survey of the literature will be included.

There are at least four conditions for the appearance of intelligent behavior in higher animals and humans. They are: (1) the evolution and presence of a cerebral cortex; (2) the evolution and presence of distance receptor organs; (3) the achievement of upright posture; and (4) the use of substitutive or symbolic behavior. First the significance of each of these four conditions will be developed and then an application to the problem of testing intelligence in infants will be made. The thesis advanced is that without the presence in some definite degree of these four conditions of intelligence there is little rationale to a testing program.

Following Darwin, a host of workers, anecdotalists with glowing stories of their favorite cats and dogs, and experimentalists working in field and laboratory, sometimes in dim attics and dusty basements, grubbing over the whole sweep of the animal kingdom, traced the structural foundations and the behavior interrelations of the phyla. The truth they discovered was that the evolution of the organism is not an evolution of structure alone, it is neural and functional as well,

for a function is only structure in action. On the surface of the brain of the reptile they found a small area covered apparently by a new kind of tissue. It was arranged in thin sheets or layers. As they followed the development of brains up the animal scale these layers became more and more prominent until among the mammals it achieved a dominating rôle over the rest of the nervous system.

With the appearance of the mammalian cortex something dramatically new in the natural history of behavior arose. Behavior lost its leaden stereotypy and its crudeness, became flexible, much more variable, and considerably refined. Animals were able to adjust to more and more delicate and complex situations. They were able to operate at a distance from objects and enemies and to delay their own reactions, the foundations of a kind of behavior which in man we call memory. Below the reptiles and birds in the evolutionary scale the simpler noncorticate animals learn with difficulty, though animal forms as far down the scale as the earthworm and even single-celled forms do learn; but the higher corticates learn easily and swiftly, some of them upon the first trial. With the possibility of animal learning and the appearance of the delayed reaction the great theological abyss between the animal and human mind disappeared. With the appearance of that insignificant patch of a new kind of nervous tissue on the brain of the reptile the structural underpinning for intelligence was being laid down. The cortex is really the first definite structural condition of the IQ.

In regard to action at a distance, consider those typical psychopaths of the animal kingdom, the amœba and the paramecium. Everybody knows that the amœba is a confirmed introvert. He lives within himself. He is a shut-in personality and he looks dumb. He has been known even to become a split personality. For his outside contacts with the environment are only occasional and then by means of his skin. He has no other sense organs, and this is more important than you think. The paramecium on the contrary is a streamlined personality. He is a dizzy manic who darts hither and yon in a drop of water, rotating on his axis and spiralling along

crazily. He acts like an extrovertive business man who has just been informed by the government of a gross mistake in his income tax. But really he is not an extrovert at all; he is only pretending, like most extroverts, to express the little that is in him, and he goes balmy trying to do it. For he too has few sense organs except his skin and the hairs upon it.

The protoplasm of these two microscopic psychopaths has not differentiated enough for the appearance of the exquisitely constructed sense receptors we see in the higher animals about us. There is no sight, no hearing. There are no organs of taste and smell. There is only the skin to admit the outside world. The best they can do as they wander about at random in their watery surroundings is to accidentally range near an object or a particle of food. If they touch it, well and good. Or if the food exhudes a chemical substance it may affect their skin surfaces. Sense organs as we know them emerge much later, after a long tortuous development through geological eons.

With the evolution of the sense organs from the common skin sensitivity of lowly single-celled creatures to the most complex structures and the remarkable sensitivity of birds and mammals, a brand new thing appeared in the world. The animal no longer needs to range alongside of an object. He can now sense it from a distance. Distant food now can be smelled; he sees his enemy before it is upon him; without even seeing his mate, its call can be heard. The new thing is the possibility of anticipating objects, events, and situations before they occur in the immediate vicinity of the organism. Such a possibility is dependent upon a type of sensory apparatus called the distance receptor, such as the eye, ear, and nose. With this equipment there came the ability to delay a response, to defer it to some future time. Distance receptors underwrite the potentialities of memory. And this is important too, because sense organs, especially distance receptors, constitute the second structural condition for the appearance of intelligence.

Interestingly enough that first patch of cortex discovered by the comparative anatomists on the brain of the reptile

was an odor cortex, and we have already seen that the sense of smell to some degree enables an organism to operate at a distance. It is not the best distance receptor but functionally it is one of them, and in its way quite efficient. The cortex and the sense of smell appear approximately at the same time in evolution. The cortex at first is an odor brain. As we go up the scale of animal life to the mammals, sight and hearing become more and more prominent. The ear develops, and with it another layered patch appears on the side of the brain. After a number of interesting attempts at producing a visual organ, the image-forming eye evolves, and with it appears a visual cortex. Sense organs and cortex go together. These are important structures in the economy of life. For eyes and ears are superior types of distance receptors. With them man can hear a telephone conversation from a distant continent, view a television scene in the next state, or see events many light years away in the depths of the Milky Way.

So not only is the distance receptor a second structural condition of intelligence, but both receptor and cortex are intimately related to each other in this capacity.

But the mammal, especially the ape, contributed another new thing in natural history which constitutes a third condition for the appearance of intelligence. This is upright posture, the simple business of standing erect on hind legs. The reptile crawls on his belly with a gliding movement. He remains close to the ground. The amphibian who was the first animal to land from the sea, was the first to walk on all fours. He was not too good at it, because he did not get his body very far from the ground. Besides, his limbs, especially its digits, are pretty crude affairs. The mammals developed quadrupedal locomotion into an art, and the anthropoid made the first attempt to get up on his hind legs. Man succeeded.

From a mechanical standpoint erect posture is a beautiful co-ordination for the resistance to the pull of gravity. It is also something very significant in human achievement. Standing up frees the forelimbs from the function of locomotion and gives the distal parts, the hands and fingers, a chance

toward a new direction in evolution. This direction finally leads to the new functions of precise reaching, of refined grasping, and of delicate manipulation of objects in the environment. And here again with this evolution there appeared a motor cortex intimately connected with the sensory areas of the cortex.

This then is the third condition of intelligence. No longer is the organism passively influenced by factors beyond its control. Erect posture is an enabling act for man to perform work, intricate kinds of work, upon the outside world. Without it there never could have been a Great Engineer in the White House.

The fourth condition is the evolution and integration of vocal cords with other structures, such as lungs, throat, tongue, lips, and teeth. For speech depends upon this organic integration. Just as upright posture enables man to manipulate the world of objects, so speech enables man to influence his fellows. The heart of the matter is that with the achievement of speech man has learned how to substitute sounds for things and persons. Animals can signal to their kind with sounds, but the substitutive response is man's own peculiar contribution to natural history. For him an intricate system of sounds becomes a set of symbols which vastly extends his own influence upon nature and upon his fellows.

The matter is so very important that it needs further clarification. In order to understand the course of mental development during childhood it is essential to recognize that much adult activity is in response, not directly to physical objects themselves, but rather to substitutes or to symbols of these objects. Thus man lives in a world of physical objects and a world of symbols for things. Clearly the symbol or sign is easier to manipulate than the unwieldly object itself. It is therefore simpler and more economical of time and energy to do the trial and error work with a set of symbols than with awkward and difficult materials. Thus a dressmaker's pattern, which is an integrated set of symbols, facilitates the making of a garment. Without it the dress either would not be made or it would be so poorly constructed that it would

not be worn. In the same sense a cookbook is a compilation of sets of symbols which eliminate or reduce the trial and error fumblings which might otherwise occur in the kitchen. All this, as will be seen later, implies a definition of a concept.

But the use of symbols in the last analysis is the highest projection and complication of the delayed response and of action at a distance. Writing is delayed speech, a radio signal is speech at a distance, a blueprint constructed in an engineer's office in New York City is delayed construction of a bridge over an African river. A symphony score, a poem, a system of calculus, are delayed substitutive responses. All of them are symbolic forms which will perform work at some future time and influence inanimate things and human lives at a distance.

The speech mechanism then is the condition upon which the use of sound symbols depends. Together with the manipulative skills which depend in turn upon upright posture, and with distance receptors all integrated by a special kind of nervous structure, the cortex, it comprises a pretty secure scaffolding to support the phenomenon of intelligence. Most of us, however, are agreed that these conditions are not quite what we mean by intelligence. What, then, is intelligence?

First and above all it is not a noun. Much less is it a noun spelled with a capital letter. Rather it is an adjective. If we would think accurately about the problem we will have to cast out this *Ding-an-sich* and talk not about intelligence but of intelligent behavior. For it is a functional or behavioral structure, really an organism in action—a certain kind of action. An intelligence test taps a child's actions, his behavior, his achievements. It evokes and gauges his number and verbal manipulations, his delayed responses or memory. It induces him to manipulate either manually, visually, or verbally such things as the parts of a picture, a row of letters, words, or numbers, or a combination of discrete objects. Always the raw data thrown up by such a test are manipulatory materials underpinned by the condition of upright posture and free prehension, or sensory materials

based upon the actions of distance receptors, in a more or less symbolic fashion, integrated into some degree of coherence and conditioned by a cerebral cortex. A mental test never taps an illusive ethereal quality. It measures behavior.

We said that intelligence is a certain kind of behavior. Society discriminates between kinds of behavior, and pronounces judgments upon them. It says that creating a symphony is intelligent behavior, while standing upright is not. It insists that blinking the eye and performing a delicate operation upon the brain lie in two widely different classes of behavior. Balancing the head, closing the fingers, withdrawing a hand from fire, turning the head toward a sound, or fixating a pretty color are simple matters of motor adjustment. These are what the physiologist calls reflexes. On the other hand, assorting a set of blocks or symbols, deciphering a code, playing the piano from a printed score, reading Aristophanes either in the original or in translation, and doing these things not only well, but easily and swiftly, these are types of behavior society terms intelligent.

Particularly we who work in the field of infant psychology are frustrated in our attempts to spot intelligence in the small baby. To tell the truth many of us have been fruitlessly chasing up the blind alley of motor and reflex development hoping against hope that some reflection of mental life might be found there. All that we did find is motor development. We can very nicely follow the course of this development during the first months and years of life but in spite of all our efforts those first months, as the Germans say, remain "*das erste dumme Vierteljahr.*" This expression, coming out of the wisdom of an ancient folklore, long ago caught an insight into our problem. We professionals are only now catching up with it. We have myopically assumed that since the adolescent IQ can be determined by a Binet scale, the same could be done for a baby by constructing a baby test. As a strictly objective venture the results have proved disappointing. One study failed to find any predictive value at all with such a scale. When the babies on whom it was used grew older the Binet test was given to them. There was no rela-

tion between the results of two tests. A recent research managed to squeeze a little juice of intelligence out of a bunch of babies, but it evaporated into thin air.

On a little thought it should have been apparent to us that when we give an infant a test made up of motor items we are tapping primarily not intelligent behavior, but for the most part the structural conditions which underlie and surround it. These conditions develop gradually in the human organism. They require time. They do not spring suddenly into existence full-blown and panoplied like Minerva from the head of Jupiter. The embryologist is able to trace the differentiation and integration of organic structures from a three-layered stage to their adult status. The process is not complete even at the end of the uterine period, for the ultimate maturity, especially of the cortex and its tracts through the nervous system, is postponed until long after birth. Just as it was the last neural organ to appear in phylogeny, so it is the last in ontogeny. It requires some time, almost a year, before it assumes a dominating role over the other nervous structures of the young child.

Until this occurs the motor behavior of the infant resembles that of the precordate animal. It is vague, much of it seemingly nonspecific, at times almost unco-ordinated and amorphous. My own name for the description of this behavior is mass activity. This does not mean the infant's behavior altogether lacks specificity. There are many clear-cut reflexes, but they are in various stages of integration with others. With the postnatal development of the cortex, this general picture will change. But during the first two or three months there are no delayed responses, few actions at a distance, little manipulative ability, and symbolic behavior is totally absent. Consequently there is small reason to test for intelligence when even the structural and behavioral conditions for it are only scantily present in the young infant.

The case of the distance sense organs is not dissimilar. Olfaction probably is the least developed of the three. The cochlea of the ear, although structurally complete at birth and connected to the central nervous system, is not well con-

nected with the cortex. The infant can discriminate gross intensities and durations of sound, but its further ability in this direction and its recognition of fine differences of pitch develop late. The eye undergoes a number of changes in its shape and growth after birth. The cornea and lens become less curved, the axis increases in length, a full complement of rods and cones at first are not present in the retina, the fovea is shallow. Pupillary action is slower than in adults and the external muscles of the eye do not co-ordinate very well. The light sense is present, that is to say, the newborn's eye responds to intensities and shows the phenomenon of dark-adaptation. Its color sensitivity is not well established although there is some indirect evidence that girls see one or perhaps two colors, but boys probably are color blind. The newborn's eye is incapable of form or pattern discrimination. It is a farsighted eye. In a word, the sense organ equipment of the young infant, especially the distance receptors, as a condition for intelligent behavior, needs considerable further maturation.

The same kind of reasoning holds true of the third condition underlying intelligent behavior, namely, upright posture. This does not occur in the human until the second half of the first year. In the meantime the infant remains a helpless victim of gravitation. He cannot sit up, much less stand up. When held upright, he does not put his foot down on the ground in the same manner as the adult. He stands on the side of his foot, occasionally upon the heel, frequently on the toes. He even attempts to stand on the top of his toes bent under. Thus his plantar traction is exceedingly poor. The long powerful extensor muscles of neck, back, and legs which resist the pull of gravity by keeping the joints at ankle, knee, and hips extended, at this time are not under the control of the nervous system. The equilibratory sense organs, although mature by the time of birth, are not properly hooked up with the muscles of the trunk and legs. If support is removed from him he flops to the floor like a rag doll. The grasping and manipulative ability of hands and fingers, which

in turn are conditioned by upright posture, is positively grotesque in its crudeness.

We previously discussed the problem of symbolic behavior and pointed out its importance in the economy of human life. It was indicated in that discussion that a complicated speech mechanism is a fundamental condition for the substitutive use of sound vibrations. This is the first phylogenetic step in the appearance and flowering of the complicated symbolic activities which is characteristic of human mental life. To this kind of behavior the name of conceptualizing has been given. But a concept is a symbol or a set of symbols together with a rule or rules for its use. This view of a concept is pertinent to an understanding of the possibility of measuring the infant's IQ, for it implies a definition of intelligence. On this basis intelligent behavior may be defined as the easy, swift, and accurate manipulation of symbols, no matter how simple or complex, according to the rules of manipulation.

Thus, creating a symphony is intelligent behavior because it involves, besides certain motor abilities, the manipulation of a set of symbols according to certain rules. Likewise, composing a lyric or reading and understanding Wittgenstein, involve, besides motor activities, the manipulation of concepts, of sets of symbols.

The physiological condition of symbolic behavior, we have emphasized, is a very complex vocal mechanism. Here it suffices to say this is an achievement coming in very late infancy. The newborn can articulate a few vowel sounds. About the third month he is senselessly babbling a number of sounds.

At a year, or soon after, this child will say true words. At about fifteen or eighteen months he will speak his first mutilated sentences. By the time he is three years old he will probably vex his parents with his constant talk and asking of questions. The use of written symbols will come much later. The further pyramiding of written and mathematical symbols into complicated patterns may require the period of adolescence and young manhood for its mastery. You will notice from this recital that none of these types of symbolic behavior

occur during the first year of life. The anlage of them, the gesture and the emotional cry and babbling, however, are present.

An analysis such as the foregoing thus clarifies the status of the four conditions for the development of intelligent behavior at the beginning of human life. During the first months these conditions, if not entirely absent, are so incipient as to thwart our efforts toward measuring intelligence with a baby test. It will be many months before they will emerge with enough distinctness of outline to constitute a supporting foundation for a superstructure of intelligent behavior.

Accordingly the infant psychologist has been hard put to it to discover real items of intelligent behavior during the early months and for the most part during the first year of life. He has found himself forced back to a selection of items which are representative rather of the conditions which underlie intelligent action, and even these he has had to tap in their rudimentary and immature aspects. The best we can do is to work out a developmental scale of motor or sensory items with which a few norms of motor development can be set up.

[MS. received July 28, 1941]

HOMEOSTASIS AS AN EXPLANATORY PRINCIPLE IN PSYCHOLOGY[1]

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Cannon, who originated the term homeostasis, defines it as meaning "a tendency to uniformity or stability in the normal body states of the organism."[2] In his various writings, and especially in his well known book, *The Wisdom of the Body* (2), he has presented an accumulation of data which point unmistakably to the existence of such a tendency in the animal organism. The thermotactic devices of the body, for example, have been found to function with great precision within the temperature range between 257 degrees above and 31 degrees below zero, Fahrenheit. Without these thermotactic devices, muscular effort alone, we are told, would produce heat sufficient to cook the albuminous substances of the body tissues. At the same time the lactic acid produced by such exercise would, without this physiological safeguard, neutralize the blood with equally disastrous results. In similar fashion the human body requires the maintenance of a constant supply of water, salt, sugar, proteins and fats. The brain, the nerves, the heart, the lungs, the kidneys, and the spleen all coöperate to check sudden and injurious changes. More recently the endocrines have been assigned a similar function.

Furthermore, the physical organism must, as a condition of survival, possess the capacity to combat invading foreign bodies and pathogenic organisms. The adaptability of this capacity to deal with novel conditions is suggestive of a form of intelligence, a fact which may have had something to do with Cannon's choice of the striking title of his book. It is

[1] Read before the Southern Society for Philosophy and Psychology, New Orlean La., March 22, 1940.

[2] The American Medical Dictionary, 1937.

not always easy, for instance, to account for the origin of such a capacity as a mere adaptation to individual or phyletic experience. Yet such a capacity seems evident from the experiments of Guyer and his collaborators, who showed that the introduction of foreign substances into the blood stream of certain animals provoked the formation of anti-bodies specifically qualified to destroy these substances, even though, presumably, the animals had never been exposed to such substances in the course of their evolution. The capacity to make novel adjustments, therefore, appears on the organic level as well as on the intellectual level of behavior. But for this capacity it is recognized that modern surgery would never have been possible.

Man's capacity to adapt himself intellectually to new experiences, as in the processes of learning especially, has been taken as a matter of course. But if we accept the validity of the current conception of body-mind as an organic whole, there is ground for assuming that Cannon's findings have quite as important implications for psychology as they do for physiology.

The evidence that homeostasis, or the tendency to maintain status, holds true of the mental as well as of the physiological aspects of organic life is not derived from mere reasoning by analogy. On the contrary there is substantial evidence pointing to the existence of this tendency at all levels of behavior, even though the data affording such evidence have not been collated with this in mind as Cannon has collated his data. Only a few attempts have been made to categorize these data under the heading of homeostasis, or to use the concept of homeostasis as an explanatory principle in psychology (4, 6, 11).

The basic facts of homeostasis, by whatever names they may have been designated, have not escaped notice. Both philosophy and science have paid heed to them. Spinoza thought he saw a connection between such phenomena and the inertia of physical bodies. The tendency of a physical body to continue a motion once begun was to him the same phenomenon as the impulse toward continued growth and

creative self-expression in human beings. Each, he said, represents the same *conatus se conservandi*, a term which might well serve as the Latin translation of Cannon's Greek compound, homeostasis. The holistic philosophy of Smuts and Whitehead's conception of the place of the human personality in the cosmos have been suggested as more modern evidences of the recognition of a common phenomenon by philosophers.

Among early psychologists, Fechner is recalled as having regarded nature as an organic whole made up of coherent systems, of which the human personality is one. In the human personality, as everywhere else in nature, there was to be found, he thought, the same primary 'tendency to stability.' Among present-day psychologists there seems to be an increasing interest in the organismic conception of the phenomena of mental life. As a matter of fact the revolt against the atomistic hypothesis and the analytical methodologists of the past can lead in no other direction.

"Even physiologists," thinks Allport, "often embrace a philosophy of 'systematic relevance.' The self-preservative, self-regulating processes of the body imply to them a root tendency to maintain wholeness. In the constant return of all psychological systems to a state of equilibrium, some see a 'wisdom of the body,' others a 'state of vigilance.' The more prosaic refer merely to 'homeostasis.' But whatever terms they employ these physiological doctrines all assume an inherent tendency of every organism to form itself into one intricate homogeneous system" (1, p. 349).

It should be borne in mind that the body states with which Cannon concerned himself in exemplifying physiological homeostasis are original, or natural states, states which are inherent in the organism itself, and are essential to its existence, such, for example, as temperature, alkalinity of the blood, the supply of water, salt, etc., previously mentioned. So far as my information goes he has not made it clear whether or not he considers that there exists the same tendency to maintain an acquired status as there is to maintain what he calls 'the normal body states.' Unless the principle of homeo-

stasis can be shown to apply to acquired states as well as to original states of the organism, it must be admitted that its explanatory value for psychology would be limited, since psychology knows very little about original states of the mental organism. However, on the assumption, previously mentioned, concerning the essential unity of all aspects of organic life, we seem justified in holding the opinion that the principle must hold good in respect to whatever original mental states there may be found to exist. But more than this, the facts are that acquired action tendencies, or habits, the drives of cultivated interests, ideational perseverative tendencies, developed personality traits, not to mention acquired drug addictions, all may without doing violence to either logic or facts be subsumed under the category of homeostasis.

Still another difference needs to be pointed out between what we are familar with as physiological homeostasis and what we are endeavoring to point to as the corresponding mental manifestation of the same tendency. Physiological homeostasis manifests itself only when the disturbing conditions are physically present, whereas, thanks to the influence in the mental equation of that unresolved and possible unresolvable, factor of consciousness, it is possible to demonstrate the tendency to maintain status at the mental level of behavior even in anticipation of the presence of disturbing conditions. Cannon himself has given many examples of physiological reactions provoked by the anticipation of disturbing conditions in developing his emergency theory of the emotions, though such reactions are not commonly thought of in connection with homeostasis responses.

Although homeostatic tendencies are demonstrable wherever organic adjustments take place, it is to be hoped that no one will be tempted to build a whole new system of psychology upon this fact. And yet it seems quite evident that Cannon's work can do something for the psychologist besides adding another technical term to his already padded glossary. To employ what Cannon, by years of productive research, has made out to be a master principle of interpretation in physiology, in the explanation of similar psychological phenomena

will, I believe, lend a new significance to many phenomena in that field.

I venture here to enumerate a few scattered instances in which, in my opinion, profitable use can be made of the principle of homeostasis in dealing with some well known, but as yet not well explained mental phenomena:

Psychology, in its consideration of the Hering theory of color vision, which is still referred to but not so much stressed as formerly, has recognized the occurrence of some sort of disturbance of physiological equilibrium in the retina, which is brought about by the exposure of the eye to light waves of different periodicities, the visual effects being produced by the disturbances. It is interesting to note that the more recent theory of Hecht also involves the concept of equilibrium. We, of course, do not know much about the nature of the original organic status, nor precisely how it is disturbed, nor yet concerning the process of recuperation. Yet we cannot but assume the existence of such a status, the disturbance of the status, and a process which brings about its restoration.

The so-called after-image of motion, which one gets after looking, say, at a rotating disk on which has been drawn a black spiral line, is a phenomenon which few writers of textbooks even attempt to explain. Again admitting that we cannot describe either the normal status or the recovery process in detail, we must admit here too the original existence of such a status, a disturbance of that status, and a tendency for it to be restored when conditions permit. It is possible that certain aspects of nystagmus may be referred to this same cateogry.

The phenomenon of 'constancy of perceiving,' whether of brightness, of color, of size, of shape, of weight, or distance, leads Feldman and Weld to conclude that much of our perceiving becomes in this way standardized. The very term 'constancy,' as applied to perceiving, suggests the operation here of homeostatic tendencies. It would be a worthwhile task to test the application of this principle in connection with visual illusions, the conventional explanations of which are neither complete nor altogether satisfactory. Especially

interesting, it seems to me, would be the examination, with this principle in mind, of the explanation of illusions in terms of the Lipps doctrine of empathy.

A. T. Poffenberger (11), who became interested in the psychological applications of Cannon's principle of homeostasis about the same time that I did, notes that R. B. Raup, in his book, *Complacency: The Foundations of Behavior* (12), attempted several years ago to reduce all habit formation, learning, reflection, and reasoning to a struggle to regain equilibrium, or complacency, as he termed it. Other pertinent data are cited by Poffenberger, such as the attempts by J. J. B. Morgan (9) to measure the effects of distracting noises on efficiency of performance. The now familiar fact that such distractions do not always decrease, but may even increase performance is taken to indicate a tendency to maintain a certain work level. H. M. Johnson, in studying the differences in sensitivity of the eye to light intensity, found that "the more unfavorable the conditions of observation the greater the degree of 'volitional attentive effort'", (7, p. 20) and, furthermore, the better the performance as measured in terms of reaction times. Poffenberger notes as an exemplification of the same principle the work of Thorndike and his collaborators on the effects of high humidity and high temperature on work efficiency. Here again the uncomfortable milieu failed to reduce the output, and in some cases increased it. The fundamental, if not primordial, character of this reaction is suggested by the fact that the physiologist Fick (3) noted many years ago that even a single muscle would contract the more strongly the greater the tension exerted upon it.

Besides work levels which an organism tends to maintain there are, according to J. D. Frank (5), 'ego levels,' and 'aspiration levels,' the maintenance of which plays an important part in the human life economy. Facts such as these bring the operation of our explanatory principle at once into the realm of the personality. E. J. Kempf claims to have recognized this fact in his monograph on *The Autonomic Functions and the Personality* (8).

It is in this realm of personality adjustments that we shall, in my opinion, find the most useful applications of the concept of homeostasis. Rationalization of one's behavior is no less an act of organic defense against ego-disturbance than is a change of blood count against infectious disease. The rise of temper against an insult is not essentially different from the rise of temperature against infection. Both represent the attempts of an organism to maintain status; in the one case it is a body status, in the other it is a social status that is to be maintained.

One only needs to recall that notable old book, Veblen's *Theory of the Leisure Class* (13), in order to bring to mind what men will do in order to achieve or to maintain social status. Adler, it will be recalled, regards neuroses as being problems of position rather than of disposition. The inferiority complex, of which he makes so much in his system of psychiatry, implies a social frame of reference, and hence a relative social status. Compensation, the major explanatory principle of his system, becomes at once much more intelligible when conceived as homeostatic defense reaction of fundamentally the same kind as those catalogued by Cannon, only on a different level of behavior. Likewise most, if not all, of the so-called defense mechanisms, which represent reactions of human personalities under conditions of disturbed status, can be better understood if referred to a single, general explanatory principle than if merely described as symptoms or set up as isolated principles in themselves.

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[MS. received May 20, 1941]

THE 'VALIDATION' OF DRIVES[1]

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The ambiguous categories of the McDougall generation have gradually given way to operationally defined concepts. This has led to the identification of drive with behavior. In line with this trend Anderson (1) distinguishes between 'drive,' by which he refers to the neural mechanism underlying behavior, and 'need,' the instigating physiological state. Thus although the hunger drive is aroused by changes taking place within the organism, this fact does not justify equating the hunger drive with these physiological conditions. He further suggests that learning may effect a shift in dependence for arousal from the original *internal* to a new set of *external* stimuli. Starting from this basic assumption, Anderson has deduced an impressive array of theorems which he is subjecting to experimental attack (2, 3). The value of this step for progress in elucidating the important relationships between motivation and learning is undeniable. A grave danger lurks, however, in assuming equivalence between operationally identical behaviors regardless of origin. As Lewin (17) reminds us, we are no more justified in postulating absolutes in the field of motivation than in that of perception. If we do we shall encounter the same difficulties that would result from assuming constancy of meaning for identifiable objects. Long ago Köhler (12) pointed out the distinction between a branch perceived as part of a tree and the same branch perceived as an instrument for obtaining food. Similarly a chair when one wants to sit down is a different 'object' from the same chair when one wants to obtain something beyond reach, It was to avoid such ambiguities that Tolman (27) introduced his concept of 'Sign-Gestalt.' In the field of motivation it is

[1] Paper read before the American Psychological Association, Northwestern University, September, 1941.

all the more necessary to recognize that a given behavioral item has different significance in the life economy of the organism according to the whole in which it is embedded. Therefore we should not regard as equivalent two examples of food-seeking behavior, one of which occurs in connection with food deficiency, the other as a result of conditioning. In other words, eating to satisfy physiological hunger has a different meaning for the organism from eating to satisfy an acquired 'social hunger.'

Unless careful evaluation of behavior is made in terms of the total situation in which it occurs, we shall find ourselves in the paradoxical position of confusing by oversimplifying, the very issues we are striving to clarify. In the writer's opinion we may avoid this pitfall by a kind of 'validation' procedure. Instances of behavior associated with vegetative drives should always be checked against some criterion of internal need. This would in no way sacrifice objectivity because our indices could be as objective and as readily quantified as the behaviors correlated with them. If, for example, food-seeking behavior were found to occur regardless of deprivation interval, or female receptivity in the 'wrong' genital phase, we should have to designate these externalized forms in terms different from those used to describe their more 'valid' prototypes.

The importance of such distinctions is shown by the widespread prevalence of externalization. Not only does this trend characterize individual development, but the literature suggests a phylogenetic parallel. In the absence of systematic comparative studies bearing on this point, we can do little more than assume increasing susceptibility to external influences in correlation with increasing capacity for learning. At the lowest levels we should expect to find the behavior appropriate to a given need more rigidly bound to that need, but as we ascend the evolutionary scale this relationship should become more flexible. We should then anticipate greater mobility of the behavior, *i.e.*, an easier shifting from the original need to the service of other needs. A survey of the experimental literature on motivation among vertebrates

lends some support to our assumption, but *proof* awaits a systematic attack on this important problem.

In the case of feeding, the influence of social as well as physiological factors has been demonstrated as far down the scale as the fishes. Welty (28), using a variety of fishes, showed that more food was consumed by groups of four than by isolated individuals. At the level of the birds, the most conclusive evidence is that of Katz (10) and his students (4, 5), who reported in the case of the hen, transposability of behavior from an originally internal need to an external stimulus situation. In some of their experiments the amount of food presented—a purely environmental factor—was the chief determiner of the amount consumed. Another external factor, the presence of other hens, also produced striking effects. The resumption of eating by satiated animals followed the introduction of hungry members of the species, and the amount of food consumed varied directly with the number of other animals present. It was by means of such findings that Katz confirmed his two-component (external-internal) theory of hunger. Among mammals, apart from Anderson's current work on externalization, Harlow (8) was able to demonstrate social facilitation of feeding in the rat.[2] He found that more food was consumed when hungry rats were fed in pairs than when alone. Competition was apparently the factor responsible for the difference. The social effect failed to appear when satiated animals were used. The same author collaborating with Yudin (9) attacked a similar problem in a group of rhesus monkeys. The results follow our theoretical expectation by showing a greater susceptibility to social influences in the higher form. This was true even in situations where competition was ruled out. In the chimpanzee, a pertinent observation reported by Nissen and Crawford (24) in their work on food-sharing, was the absence of correlation between begging for food and degree of hunger. The importance of social factors in the feeding behavior of humans is a matter of common observation. The multiplicity of motives served by eating and drinking does not

[2] Drinking in the rat has also been found subject to social facilitation (6).

demand an excursion to the Kwakiutls but may be discovered directly by introspection.

Another line of evidence suggesting a loose tie between nutrition and feeding is adduced by Levy (13, 14, 15, 16). He reports that under conditions where sufficient food was obtained but with a curtailment of eating responses, the latter were continued long after physiological hunger was satisfied, and appeared in pathological forms. His material included pecking in chicks, licking in dogs, and thumb-sucking in babies.[3]

We have chosen our examples from different phylogenetic levels in the hope of revealing a trend toward increasing susceptibility to social factors. Such a trend may be more clearly shown in forms of behavior that are not vital for individual survival. In life-preservative functions, *e.g.*, nutrition, one may expect a closer association with physiological conditions, and consequently less in the way of environmental modification. Other forms of behavior which subserve a vegetative but not a vital function, however, may be more easily shaken from their somatic moorings and subjected to outside influences. Sexual behavior affords the best illustrations of this trend and most clearly reveals the difficulties in which we may become involved unless we keep constantly before us the setting in which the behavior occurs. In an effort to find more general confirmation of Katz' two-component theory, Skard (25) set up a series of experiments on sexual need in hens which roughly paralleled the earlier ones on hunger. The results indicated extra-somatic factors in sexual motivation. For example, increasing the amount of sexual stimulation led to greater sexual indulgence, just as increasing the the amount of food presented had led to a continuation of eating beyond the point of physiological replacement. Moreover, thwarting of other needs, *e.g.*, lack of food, led to an intensification of sexual zeal, the sexual indulgence thus becoming what one might be tempted to call, 'sublimated eating'!

[3] That the relation between food need and eating is variable may also be shown in converse by the many instances where hunger does not induce eating even in the presence of food. The most frequent cause of this anomaly is conflict with other drives.

In the lower mammals represented by various rodents, sexual behavior seems to be closely associated with hormonal conditions, especially in the female. In the cat, on the other hand, Winslow (29) reported situations in which male and female copulatory positions were assumed as expressions of dominance-subordination status, and apparently were devoid of sexual significance.

It is at the primate level, however, where transposability of sexual behavior reaches its peak and confusion of interpretation is rife. It has been noted by many investigators that sexual behavior is frequently used to mediate non-sexual motivation. Hamilton (7) recognized its rôle in connection with escape from an enemy. Kempf (11) saw in it a means of procuring food and protection. In an attempt to bring order out of chaos, Maslow (19, 21, 22), like Katz, set up a two-factor theory. Working with a variety of primates, he distinguished between sexual behavior which is a direct product of physiological tensions, and sexual behavior which expresses the impulse to dominate, and is used as a 'power weapon.' He pointed out that it is often interchangeable with bullying and fighting to subdue an annoying partner. This theory would explain why primate sexual behavior apparently occurs independently of periodicity although hormonal cycles can be demonstrated. It also explains the frequent appearance of various perversions that do not seem to have an endocrine basis. Recently, Yerkes (30) and Yerkes and Elder (31) have been able to reveal in the chimpanzee a period of 'heat' which is correlated with a condition of genital skin swelling. Copulation is likely to be limited to this time in the case of experienced and congenial consorts mated under controlled conditions. It does not, however, necessarily imply receptivity in the female, and may occur regardless of œstrous phase as a defensive or accommodating reaction to a dominant male.

Our knowledge of the primate should guide our interpretation of human sexual behavior. But again we find that the tendency to isolate a given behavioral item from its total motivational context has beclouded the issue. Malinowski

could dichotomize man and animals thus: "The sexual impulse is not confined to any season, not conditioned by any bodily process. . . . It is there to affect at any moment the life of man and woman . . ." (18, p. 198). "With the animal instinctive endowment and physiological change throw male and female into a situation out of which they have to extricate themselves by the simple play of natural impulses, with man the control comes, as we know, from culture and tradition" (18, p. 199). Miller (23) attempted to clear things up by pointing out the relation between the reproductive behavior of man and that of the lower mammals. Tinklepaugh (26) also stressed the continuity in mammalian sexual behavior. He reminded us that psychological factors are so marked in the human that they obscure a biological cycle which is really homologous with the œstrous cycle of lower forms. He also made the interesting suggestion that demonstration of affection has developed by conditioning so that it readily leads to sexual stimulation and coitus. This is another way of saying that the human sexual drive has become externalized.

These illustrations which we have presented from food-seeking and sexual behavior may suffice to indicate the danger of identifying drive with isolated behavior. We have noted in the course of development of the species as well as in that of the individual, a tendency for drives to become externalized. This process involves an increasing readiness for transposability, *i.e.*, for the behavior originally associated with one drive to become elicited in the service of another. An integrative drive like dominance (20) for example, may utilize as channels of expression, the behaviors properly belonging to the 'simpler' drives of hunger and mating. To avoid confusion and to achieve clearer insights we have suggested validating each behavior item and interpreting it in terms of its total setting.

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[MS. received June 16, 1941]

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